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No. I.

PROFESSOR CAYLEY, F.R.S., President, in the Chair.

Edward Alfred Hadley, Esq., 31 Pembroke Road, Kensington,
was balloted for and duly elected a Fellow of the Society.

List of Co-ordinates of Stars within and near the Milky Way.
By A. Marth.

(Communicated by W. Lassell, Esq.)

I send you the first half of the list of Co-ordinates of Stars within and near the Milky Way, which I mentioned to you in some previous letter. Will you now be good enough to communicate it to the R. A. S., and to make a friendly appeal to those fellows of the Society, who are skilled draftsmen, to devote their skill to the production of a trustworthy sketch of the Milky Way? I need not explain to you at great length the object of such an appeal, as you will understand it at once, even without calling to remembrance old Malta discussions. You know better than most observers how little many of the published drawings of nebulae and other bodies can be depended on as faithful representations of

the objects in the heavens, which they pretend to represent ; you know how unsatisfactory and doubtful, in some cases, the evidence is which such drawings furnish ; how puzzling the attempt of tracing any fair resemblance, and of accounting for the presence as well as for the absence of prominent features ; how perplexing the task of discriminating between avoidable and unavoidable exaggerations. At the same time you are too well acquainted with the manifold difficulties in the way of producing and also of multiplying trustworthy sketches, not to be ready to make great allowance for defects and shortcomings, which may sometimes be due to causes and circumstances little dreamt of by the uninitiated. Now, the remedy for getting rid of, at least, a great part of the uncertainties and perplexities in the correct interpretation and appreciation of drawings is obvious. If those who furnish telescopic sketches, will only take the trouble to sketch in just the same way some kindred object in the heavens, which can easily be referred to, either without or with very little telescopic aid, it will be feasible, by comparing the sketches with the object, to determine their merits and demerits, and to estimate accordingly the degree of trustworthiness and the true value of the telescopic sketches made by the same hand, with the same care, in the same manner.

When (now nearly seven years ago), I discussed with Mr. Newall the different branches of astronomical research to which his great telescope ought to be devoted, so that science might derive the greatest benefit, I proposed to him with respect to drawings, and he readily agreed, that whoever might undertake the drawing of nebulae or comets and planets, as seen through the great telescope, should also have to make similar drawings of, at least, a portion of the Milky Way, and also of the Moon as seen by the naked eye, or through a mere opera-glass, so that the latter drawings might serve as a fair test and index of the trustworthiness of the others. I also proposed that the drawings themselves should be made by the most skilful hands available, but that it should be the astronomer's business to furnish the correct places of all the leading points, so that the drawings should all be made to scale and without any sensible distortion. I need not remind you that the execution of this preliminary work, if it is to possess a high degree of accuracy and to be extended to a great number of objects, is of itself a very serious undertaking, which can only be accomplished in a satisfactory manner where sufficient favourable opportunities are afforded for working at it. But since the great refractor, if its power to serve the interests of science and to enlarge our knowledge of the heavens be not paralysed, was of necessity to be erected in some suitable locality in a more southerly latitude, with a pure atmosphere and plenty of clear skies, the plan was quite practicable of making these accurate measurements of details, together with a systematic general review of the heavens for nebulae, &c., a part of the regular observing work. For it could be reconciled very well with the more pressing claims of the satellites and other urgent demands, full allowance being,

at the same time, made for those (in the case of a private observatory, of course, very legitimate ones), which require the instrument, not as a scientific tool, but merely as a means for satisfying curiosity. From Mr. Newall's letter in the *Astronomical Register* for July of last year, you know that the conditions have changed considerably, and that consequently any such comprehensive work is now out of the question. My plans for accomplishing it were made on the supposition of a favourable locality; they were quite clear and distinct, as they were based upon considerations of very practical experience; if they aimed somewhat high, they aimed at nothing vague, or fanciful, or indefinite; they aimed only at what, under the contemplated conditions, would have been fairly feasible; they presumed no permanent assistance, except the indispensable mechanical assistance of a trusty workman, though they readily admitted any useful scientific help which might be available, and at the same time were compatible with the limited employment of the great telescope in other branches of scientific research, provided workers could be found. But then, these plans certainly assumed as an essential condition, that the observatory should be erected in some really favourable locality, Malta, Teneriffe, Madeira, any place, in other respects not too disadvantageous, where instrument and work would have a fair chance. In the absence of this essential condition the case is, of course, entirely altered. Anything like such comprehensive work, as I had in view, is now quite hopeless; all that can be aimed at is to secure for exact science what little the circumstances may permit. But how much that little will amount to, what small portion of the harvest may possibly be saved, which would be within reach if the tools were only put in the proper field, cannot at present even be conjectured, till it is decided where and when, and in what condition, the observatory will be put in working order.

However, though even at the best, for want of opportunity, accurate measurements of details may now have to be restricted to a very few selected cases, the drawings as drawings may perhaps get the advantage of being executed by very superior skill, and it is partly with a view of discovering the superior special skill amongst our amateurs or artists, that I send you in the enclosed paper the correct places of the leading stars for a map of the Galaxy, and beg you now to make a friendly appeal to those interested, to try whether they cannot manage to produce something like a fair and truthful representation of what they see, when they look up to the Milky Way in the heavens. If the task is in some respects a difficult one, it is for competent hands certainly worth attempting, and it is perhaps just the kind of task which some amateurs will like. It does not demand any previous knowledge of astronomy or astrognosy, beyond the trifling practical acquaintance with some leading group of stars, say the W. of *Cassiopeia*; all the further knowledge required may be gathered from the heavens themselves with the help of a star-map, con-

structed according to the list. But the task demands special technical qualifications, the possession of which puts it into the power of any one, who will take the necessary pains, to render a substantial service to astronomical science, by supplying a long-felt want, which up to the present time has remained unsatisfied.

As regards the actual state of our knowledge of the Milky Way, I need only refer to Argelander's remarks in his old 'Appeal to Amateurs' ('Aufforderung an Freunde der Astronomie,' &c., in Schumacher's *Jahrbuch für* 1844, pp. 122-254), where the subject is treated pp. 180-185, of which, should you wish it, I will send you a translation, if it does not already exist somewhere, or if your daughters do not prefer to furnish one. Since the time of Argelander's appeal Sir John Herschel's drawing of the southern portion of the Milky Way has been published in his *Cape Observations*; and Heis in Munster, and Schmidt in Athens, have been engaged in preparing sketches of the portions visible from their stations. Herschel's drawing, however, can, for obvious reasons, only be looked upon as a highly valuable first attempt. Heis's long-expected new star-maps are not yet published, but from a remark in the report to the meeting of the Astron. Gesellschaft held last year at Stuttgart, it would seem, that Heis himself is not quite satisfied with the technical representation of the Milky Way, which they contain (v. *Vierteljahrs-schrift der Astron. Gesellschaft*, vol. vi. p. 267). Whether anything more is publicly known about Schmidt's drawing than what is mentioned in the report of the meeting held in 1867 at Bonn (v. same publication, vol. ii., p. 211), I am unable to say. But suppose even that Schmidt's chart is to be published before long, it will obviously be of advantage to science to possess several independent representations, made by different hands, under different as well as under similar conditions, and I should recommend an appeal to qualified draftsmen for such representations, even if they were not intended to serve at the same time the special purpose which I mentioned first.

It seems unnecessary to offer here any suggestions respecting the technical execution of the drawings. Of course, an object so complicated as the zone of the Galaxy, comprehending such manifold details and such great varieties of light, from very bright sharp points down to the faintest nebulous patches, cannot be represented on paper without a considerable amount of conventional exaggeration; and the most effective or the most practical manner of doing so is not likely to be found except after repeated trials and critical discussions. The only question which it is desirable to settle beforehand is the question of scale, and accordingly, with a view to promote agreement and at the same time, perhaps, to facilitate the insertion of the stars in their correct places, the co-ordinates given in the list are expressed in inches and hundredths, at the rate of one inch to five degrees, which, for the present purpose, will probably be found the most suitable and convenient scale. The whole map of the galactic zone will then have

a length of six feet, consisting of, say, six sections, each twelve inches long and eight inches broad, the central six inches of which should show nothing but the representation of what is seen in the heavens, while the network of the map, and any letters, names of constellations, marks of reference, &c., should be confined to the two outside inches, where they do not encroach upon the drawing. The list, the first half of which I send you, furnishes the rectangular co-ordinates x and y (x being reckoned from right to left, y downwards), of all the leading stars down to the magnitude 5-6 of Argelander's *Uranometria Nova*, in the central zone of the map, or between the galactic polar distances 75° and 105° , and also of a considerable number outside these limits. The fainter stars visible to the naked eye are then to be inserted by estimation, just as in drawings of nebulae it is the case with stars too faint for being measured micrometrically. I am quite willing to furnish in a supplementary list the co-ordinates for a number of additional stars along the borders of the Milky Way, or in other critical positions, but for the purpose I must first learn which they are.

The magnitudes adopted in the list are chiefly those of the catalogue of the *Uranometria*, the old notation for intermediate magnitudes being, for convenience, supplanted by the decimals $\cdot 3$ and $\cdot 7$, so that $4^{\text{m}}\cdot 3$ must be understood merely to mean a faint 4^{m} , and $4^{\text{m}}\cdot 7$ a bright 5^{m} . The pole of the system of galactic co-ordinates, I have assumed to be in R. A. 190° , and Decl. 60° for the equinox of 1880. The galactic longitudes and polar distances have then been computed independently for every star in the list, so that the last figures of the co-ordinates there given can be relied on. You may, perhaps, think, that I have taken more pains in preparing this preliminary work than the subject deserves. Though an excuse will scarcely be needed, it may be just as well to explain, that the satisfactory settlement of the question concerning the appearance of the Milky Way is merely part of the auxiliary and preparatory work in connexion with the systematic general review of the heavens, to which I alluded before, and which I should have undertaken with a fair prospect of practical success (so far as the latitude of the observatory might allow), if the opportunity had been granted. Our Malta experience has sufficiently proved how much and extended critical re-examination of the heavens is wanted, but it has also shown that, even under favourable circumstances, a series of years will be required for accomplishing it in a satisfactory manner.

The second half of the list of co-ordinates, comprising the southern portion of the Milky Way, is at your service whenever you like, as I have it all ready, save some possible deficiencies occasioned by the want of a trustworthy map or catalogue, from which the correct magnitudes of the lucid stars not visible in our latitudes might be taken. If Behrmann's estimations were known, any uncertainty in selecting the stars required would probably be removed; but, so far as I am aware, they are not yet published.

Such an uncritical compilation, as the B. A. Catalogue, is, of course, too precarious a guide and cannot be depended on, though, from the way in which it has been compiled, it may be safely presumed that it will be found considerably less incomplete in the regions south of the tropic of Capricorn (the northern limit of Lacaille's Catalogue), than in those north of it.

Co-ordinates of Stars within and near the Milky Way.

x in.	y in.	Mag. in.	Star.	B.A.C.
0°31	3°75	5	4 Aquilæ	6379
°57	1°47	5	71 Ophiuchi	6142
°59	6°22	5·3	e Aquilæ	6679
°66	2°55	cl.	viii. 72	(6280)
°72	1°40	3·3	72 Ophiuchi	6143
0°96	3°96	3·7	♄ Serpentis	6460
1°01	6°34	4·3	i Aquilæ	6715
°04	5°74	5	" "	6653
°10	7°24	5	" "	6815
°50	5°41	3·3	♄ "	6646
°60	4°41	5·3	19 "	6552
1°77	6°82	var. 3°5-4°7	" "	6811
2°24	5°77	5	σ "	6729
°42	3°82	5	18 "	6543
°45	5°35	4·7	μ "	6701
°67	3°24	5	11 "	6483
°84	6°35	4	β "	6833
°86	3°48	3	ζ "	6528
2°94	3°12	4	ε "	6487
3°00	4°50	5·3	♄ "	6644
°14	0°78	4	109 Herculis	6251
°14	5°90	1·3	α Aquilæ	6802
°17	6°09	5	ξ "	6825
°19	2°29	4·3	111 Herculis	6397
°31	2°84	5	" "	wanting
°32	5°53	3	γ Aquilæ	6772
°59	2°01	4	110 Herculis	6387
°73	5°86	5·3	φ Aquilæ	6838
3°89	2°20	5	112 Herculis	6438
4°16	2°19	4·3	113 "	6453
°36	4°59	4·3	β Sagittæ	6744
°37	3°71	5	4 Vulpec.	6654
°43	3°18	4·7	1 "	6589
°43	4°49	4·3	α Sagittæ	6739
°59	4°08	5	9 Vulpec.	6709

x in.	y in.	Mag. in.	Star.	B.A.C.
4.70	4.70	4	δ Sagittæ	6783
.70	1.47	5	..	wanting
.75	7.37	4	ε Delphini	7088
.84	4.73	5	ζ Sagittæ	6794
4.86	6.17	5	η Aquilæ	6952
5.16	5.07	3.7	γ Sagittæ	6858
.29	3.35	4.3	6 Vulpec.	6674
.38	7.05	4.7	ζ Delphini	7107
.41	5.27	5.3	η Sagittæ	6901
.43	2.95	5.3	3 Vulpec.	6657
.43	7.15	3.3	β Delphini	7121
.48	4.43	5	12 Vulpec.	6810
.69	7.31	4	δ Delphini	7173
.72	7.07	3.7	α ..	7149
.79	4.36	4.7	13 Vulpec.	6827
.87	1.10	5.3	ν Lyræ	6427
.90	3.08	3	β Cygni	6690
.96	7.30	3.3	γ Delphini	7200
5.99	1.51	5.3	λ Lyræ	6497
6.02	1.03	var. 3.5-4.5	β Lyræ	6429
.04	2.66	5	2 Cygni	6648
.05	4.92	5.3	17 Vulpec.	6912
.07	1.43	3.3	γ Lyræ	6491
.15	4.59	5	16 Vulpec.	6883
.18	3.78	3?	Nova of 1670	
.20	1.76	5.3	17 Lyræ	65
.47	3.18	5	ϕ Cygni	6740
.54	6.38	5	29 Vulpec.	7140
.59	4.23	5	15 ..	6879
.60	5.00	5	16 H ..	6966
.73	0.48	4.3	ζ Lyræ	6392
.76	0.89	4.3	δ ..	6466
.83	0.10	1	α ..	6355
.86	1.45	5	ϵ ..	6556
6.97	4.76	5	23 Vulpec.	6973
7.01	6.02	5.3	28 ..	7143
.04	2.46	4.7	8 Cygni	6698
.11	0.31	4	5 ϵ^2 Lyræ	6391
.12	0.30		4 ϵ^1 ..	6390
.20	3.29	var. 4-13	χ Cygni	wanting
.24	3.05	5.3	17 ..	6784
.26	2.07	5	4 ..	6667
.39	1.56	4.3	θ Lyræ	6599
.53	1.37	4.3	η ..	6581

<i>x</i> in.	<i>y</i> in.	Mag. in.	Star.	B.A.C.
7°69	3°24	4·3	η Cygni	6851
°74	4°92	4·3	41 "	7067
°81	2°59	5·3	15 "	6771
°83	4°45	5	39 "	7029
7°87	6°11	5	31 Vulpec.	7246
8°09	6°06	5·3	32 "	7256
°20	4°00	5·3	35 Cygni	6998
°24	5°42	4·3	52 "	7194
°25	2°85	5·3	22 "	6849
°28	3°48	5	28 <i>b</i> ² "	6937
°39	3°65	5	29 <i>b</i> ³ "	6967
°52	4°55	5·3	47 "	7103
°67	3°61	var. 3 < 6	P "	6990
°75	5°01	2·7	ε "	7204
°84	4°99	var. 5-6	τ "	
8°97	6°36	3	ζ "	7368
9°13	3°49	2·7	γ "	7022
°17	1°80	3	δ "	6779
°17	4°72	4·7	λ "	7213
°79	7°50	4·3	μ "	7568
°79	5°84	4·3	ν "	7399
°93	1°05	4·7	θ "	6734
°99	4°98	5·3	61 "	7336
9°99	2°46	4	31 <i>o</i> ¹ "	6965
10°10	0°72	4	ι "	6697
°12	4°38	4	ν "	7277
°14	5°30	4	τ "	7385
°19	2°40	4·3	32 <i>o</i> ² "	6983
°35	3°41	1·7	α "	7171
°37	3°80	5·3	56 "	7241
°41	5°18	4·3	σ "	7398
°50	3°84	5·3	57 "	7253
°63	1°35	5	κ "	6856
°65	1°16	5·3	ι "	6824
°68	2°64	5	45 <i>ω</i> ¹ "	7085
°72	4°21	4	ξ "	7333
10°79	5°76	5	72 "	7505
11°03	6°02	5·3	79 "	7566
°07	4°55	5	A "	7402
°11	5°53	5	74 "	7521
°11	3°58	5·3	<i>f</i> ¹ "	7301
°28	7°48	4	π Pegasi	7731
°29	3°73	5·3	<i>f</i> ² Cygni	7345
°38	5°56	5·3	..	7565

<i>x</i> in.	<i>y</i> in.	Mag. in.	Star.	B.A.C.
11.50	0.78	5.3	23 Cygni	6847
.63	1.34	4.3	33 "	6976
.70	4.67	4.3	ε "	7503
.70	4.42	5	g "	7480
.85	3.76	5.3	..	7411
11.93	4.03	5	..	wanting
12.02	6.87	4.7	1 Lacertæ	7777
.02	4.20	cl.	Mess. 39	(7496)
.17	6.49	5	1 H Lacertæ	7765
.33	1.91	4.7	6 H Cephei	7215
.50	4.37	4.3	π ² Cygni	7598
.62	3.98	4.7	π ¹ "	7560
.96	0.94	4	θ Cephei	7098
.97	7.10	5	10 Lacertæ	7901
12.99	1.38	3.7	η Cephei	7220
13.08	6.23	5	6 Lacertæ	7850
.13	5.47	4.7	2 "	7800
.31	4.57	5.3	..	7746
.51	5.42	5	5 "	7845
.51	6.20	5	11 "	7906
.54	5.03	5	4 "	7820
.60	2.82	var. 4-5	μ Cephei	7582
.65	1.84	2.7	" "	7416
.79	4.54	4.3	3 Lacertæ	7815
.81	5.00	4	7 "	7855
.92	2.22	5.3	9 Cephei	7542
13.93	2.48	5	ν "	7595
14.06	6.89	3.7	ο Andr.	8023
.08	3.59	4.7	ε Cephei	7778
.10	4.86	5	9 Lacertæ	7888
.11	3.33	3.7	ζ Cephei	7749
.55	3.54	var. 3.7-4.9	δ "	7848
.69	2.17	4.7	ξ "	7700
.74	5.49	5.3	3 Andr.	8036
	0.83	3	β Cephei	7493
14.95	5.71	5	7 Andr.	8082
15.08	5.84	5.3	8 "	8114
.20	0.87	5	11 Cephei	7588
.24	2.74	5.3	30 "	7902
.43	7.15	4	ι Andr.	8229
.51	3.78	5.3	1 Cass.	8054
.56	6.96	4	κ Andr.	8237
.58	6.52	4	λ "	8224
.69	0.97	4.7	24 Cephei	7758

<i>x</i> in.	<i>y</i> in.	Mag. in.	Star.	B.A.C.
15.69	2.38	4.3	ι Cephei	7967
15.87	6.61	5	ψ Andr.	8261
16.02	4.14	5	ι H Cass.	8188
.16	0.95	5	31 Cephei	7896
.45	4.23	5	τ Cass.	8268
.48	5.72	var. 4.8 < 1.2	R "	
.59	4.50	5	ε "	8310
.66	4.86	5	σ "	8330
.70	6.83	5.3	22 Andr.	16
.72	0.82	4.7	π Cephei	8074
16.88	2.45	6	41 H "	8273
17.03	4.24	2.3	β Cass.	7
.23	0.51	3.3	γ Cephei	8238
.51	3.28	3.3	Nova of 1572	
.56	5.22	5	λ Cass.	121
.68	3.54	4.3	κ "	126
.71	5.35	4	ξ "	153
.82	4.82	var. 2.2-2.8	α "	169
.85	7.88	neb.	Neb. Andr.	184
17.93	6.47	5	ο Cass.	198
18.05	4.56	3.7	η "	218
.07	5.94	5	ν "	219
.13	7.91	4.3	υ Andr.	227
.24	3.98	2	γ Cass.	253
.59	5.11	6	μ "	314
.69	5.06	4.3	θ "	339
.71	2.45	5	ψ "	412
.79	6.65	4.3	φ Andr.	330
.80	7.31	5	41 "	318
.86	4.47	5	φ Cass.	391
.87	0.54	5.3	47 "	597
18.96	4.01	3	δ "	416
19.15	1.47	4	50 "	600
.22	1.77	4.7	A "	595
.24	2.22	5	ω "	568
.29	6.93	5	ξ Andr.	404
.48	3.20	3.3	ε Cass.	564
.48	6.93	5	ω Andr.	432
.72	6.23	3.7	υ Persei	487
.81	5.79	4	φ "	522
.89	0.71	4.7	37 H Cass.	955
.92	2.26	4	ι "	744
.94	7.05	5.3	κ Andr.	492
19.98	7.65	4.3	υ "	480

<i>x</i> in.	<i>y</i> in.	Mag. in.	Star.	B.A.C.
20°17	4°91	5·3	4 Persei	614
·45	4°26	cl. vi.	33 <i>h</i> "	(700)
·52	4°22	cl. vi.	34 <i>h</i> (χ) <i>Persei</i>	(718)
·74	0°86	4·3	9 H Camelop.	11137
·80	0°93	5	..	wanting
·95	7°22	2·3	γ Andr.	628
20°99	2°10	5	1 H Camelop.	1001
21°12	5°47	5	65 Andr.	735
·14	6°09	5·3	<i>c</i> "	706
·16	6°75	5·3	<i>b</i> "	676
·35	4°14	3·7	η Persei	863
·50	1°44	5	7 H Camelop.	1144
·63	3°80	5	<i>h</i> Persei	948
·73	4°65	4	τ "	885
·76	5°43	4	θ "	827
·82	2°92	4·7	2 H Camelop.	1058
·94	4°37	3	γ Persei	947
·95	4°63	5	..	918
21°95	3°09	5	3 H Camelop.	1062
22°24	1°97	cl.	vii. 47	wanting
·26	6°62	cl.	Mess. 34	(822)
·31	0°69	4	9 Camelop.	1474
·35	3°65	5	4 H "	1065
·43	4°98	4	<i>i</i> Persei	962
·50	4°63	5	..	995
·50	7°08	5	12 "	821
·64	4°71	5	29 "	1007
·67	4°72		31 "	1011
·83	4°67	2	α "	1043
22°95	5°78	4·3	κ "	967
23°01	4°63	5	34 "	1066
·13	6°88	5	π "	912
·22	4°86	5	σ "	1071
·32	6°47	var. 2°3-4°0	β "	963
·35	4°71	5	ψ "	1099
·43	1°22	4	10 Camelop.	1536
·44	6°90	var. 3°4-4°0	ϵ Persei	953
·48	5°81	5	<i>l</i> "	1026
·56	3°91	5·3	A "	1214
·57	6°64	5	ω "	981
·57	4°65	3	δ "	1129
·62	3°07	5	12 H Camelop.	1293
·71	1°33	5	11 "	1546
23°84	3°76	4·3	λ Persei	1254

x in	y in.	Mag. in.	Star.	B.A.C.
24°12	3°51	5	δ Persei	1301
25	4°10	4	c "	1266
28	5°42	4	v "	1139
30	3°86	4·3	μ "	1287
41	2°16	5	7 Camelop.	1504
73	3°98	5	d Persei	1323
93	2°19	5	9 Aurigæ	1554
24°98	5°51	3·3	s Persei	1219
25°28	6°84	5	o "	1132
33	0°56	4·3	δ Aurigæ	1885
40	5°01	5	f Persei	1291
57	7°05	4	o "	1138
57	6°12	4	ξ "	1228
86	4°31	5	e "	1414
25°95	6°84	3	ζ "	1207
26°04	2°59	1	α Aurigæ	1613
08	3°27	var. 3°5-4°5	s "	1540
52	3°59	4	ζ "	1541
59	3°45	3·7	η "	1558
80	8°20	3	η Tauri	1166
87	1°32	5	π Aurigæ	1897
26°96	4°44	5	z "	1492
27°04	1°43	2	β "	1895
05	3°21	5	λ "	1631
44	6°71	5·3	ϕ Tauri	1326
62	4°74	3	i Aurigæ	1520
86	2°34	5	τ "	1830
27°91	2°27	4	v "	1845
28°08	3°49	5·3	19 "	1636
13	4°03	5	14 "	1627
17	4°20	5·3	14 "	1614
17	3°58	5·3	ϕ "	1690
22	2°47	5	v "	1844
32	7°13	4·7	v Tauri	1367
37	7°23	4·7	x "	1362
41	2°19	3	θ Aurigæ	1900
67	3°66	5	χ "	1723
73	6°62	4·3	τ Tauri	1449
28°96	7°53	3·7	s "	1376
29°11	4°29	2	β "	1681
62	5°97	5	i "	1551
62	7°61	1	α "	1420
72	6°74	5·3	i "	1493
29°93	3°41	5	136 Tauri	1863

<i>x</i> in.	<i>y</i> in.	Mag. in.	Star.	B.A.C.
30°01	0°57	3·3	♄ Gemin.	2237
°09	2°38	4·7	♋ Aurigæ	2001
°23	6°16	5·3	♉ Tauri	1568
°32	3°40	5·3	139 "	1896
°34	3°90	5·3	132 "	1837
°63	4°71	3·3	ζ "	1767
°68	6°65	5	11 Orionis	1557
°80	6°43	5·3	15 "	1591
°89	3°12	cl.	Mess. 35	(1962)
30°92	3°44	5	1 Gemin.	1938
31°20	4°14	4·7	54 χ ¹ Orionis	1876
°31	3°09	3·3	η Gemin.	2002
°46	3°77	5	62 χ ² Orionis	1939
°48	1°68	3·3	ε Gemin.	2194
°49	2°77	3	μ "	2047
31°50	5°07	5	126 Tauri	1792
32°03	2°75	4·7	ν Gemin.	2090
°37	5°14	5·3	134 Tauri	1846
°46	6°03	3·3	λ Orionis	1749
°47	4°17	4·7	ν "	1958
°53	6°09	5	φ ¹ "	1748
°62	6°03	4·7	φ ² "	1766
°67	4°05	5	ξ "	1990
°75	1°26	var. 3·7-4·5	ζ Gemin.	2305
°81	6°83	2	γ Orionis	1687
°91	2°75	2·3	γ Gemin.	2163
32°98	0°46	3·3	δ "	2410
33°12	4°06	5·3	κ Orionis	2017
°22	4°92	4·7	μ "	1928
°42	5°45	var.	α "	1883
°57	6°47	5	ω "	1782
°61	2°79	5	30 Gemin.	2199
°62	3°48	5	..	wanting
°69	2°79	3·7	ξ Gemin.	2206
°80	1°03	3·7	λ "	2398
33°87	2°34	5	ε "	2255
34°11	3°27	var. 4·9-5·6	♁ Monoc.	2185
°17	7°24	var. 2·2-2·7	δ Orionis	1730
°35	7°77	3·3	η "	1730
°38	3°84	4·7	13 Monoc.	1684
°44	7°15	2	ε Orionis	1765
°60	3°15	5	17 Monoc.	2216
°62	4°50	4·7	8 "	2059
°69	7°02	2	ζ Orionis	1794

x in	y in.	Mag. in.	Star.	B.A.C.
34°76	7°18	3·7	σ Orionis	1780
·79	4°11	5	12 Monoc.	2123
34°90	0°93	5	6 Can. Min.	2473
35°08	7°54	4·7	c Orionis	1759
·18	7°60	4	δ' "	1758
·28	7°65	3	" "	1762
·30	1°37	5·3	ϵ Can. Min.	2451
·38	6°30	5·3	..	wanting
·42	1°29	5	γ Can. Min.	2468
·52	1°39	3	β "	2462
·60	3°67	5	18 Monoc.	2222
35°83	4°74	5·3	..	wanting

The Rich Nebular Regions in Virgo and Coma Berenices.

By Richard A. Proctor, B.A. (Cambridge).

In the maps which illustrate my papers on the distribution of the nebulae, in Vol. xxviii. of the *Monthly Notices*, the nebulae are not shown in their true places, but are simply arranged according to their numerical distribution in spaces having given limits in R.A. and Dec. While engaged in the construction of these maps, it occurred to me that considerable light might perhaps be thrown on questions relating to stars and nebulae, if maps were constructed on an adequate scale, showing the stars down to telescopic orders of magnitude, and all the known nebulae, properly placed. It is clear that to construct such maps for the whole heavens would be a work involving several months of labour, and it is seldom that I can find even a few hours of leisure for such work. I have, however, begun to prepare maps of the most interesting nebular regions, and I now present two such maps—one showing the rich nebular region in the wing of *Virgo*, the other the rich region in *Coma Berenices*. The former map contains upwards of 250 nebulae, the latter containing some 80; and in both maps all the stars in Argelander's series of charts (corrected for precession, so as to accord with the date of Sir John Herschel's Catalogue of Nebulae) have been included. Thus the maps show all the stars down to Herschel's 11th magnitude, or to the magnitude intermediate to Argelander's 9th and 10th.*

* I say down to these magnitudes; but there are many who prefer to speak of the fainter orders of stars as belonging to the higher orders of magnitude. It would be well that some definite rule should be adopted in this matter. My own sole reason for speaking of the brighter orders as the higher orders is that I find that mode of speaking to be more in accordance with ordinary ideas on the subject than that which calls a very faint star a star of a high order of magnitude. Authority is divided on the subject, some astronomers adopting one mode of speaking, while others use the other. Hence some confusion is apt to result.

The Nebular Region in *Virgo*.

The larger map extends from $12^{\text{h}} 0^{\text{m}}$ to $12^{\text{h}} 45^{\text{m}}$ in R.A., and from the equator to 20° in north declination. Thus it covers about $\frac{1}{10.5}$ th part of the whole heavens, and the average proportion of nebulae for that extent of surface would be about 28. Since it contains more than 250, its relative richness is unmistakable.

The smaller region extends from $12^{\text{h}} 32^{\text{m}}$ to $13^{\text{h}} 12^{\text{m}}$ in R.A., and from 23° to 38° in north declination. In the most northerly part of the map, however, the range in R.A. is about a degree and a half greater. The area is about $\frac{1}{36.0}$ th part of the heavens. The average proportion of nebulae would be 15, the actual number is more than five times as great.

In drawing these maps, I have been much struck by a circumstance which may perhaps not seem particularly well marked on a casual examination of the maps, but which has a real existence nevertheless. The stars are not arranged uniformly over either region, but to some degree clusteringly with interspersed spaces relatively vacant. Now no nebulae appear in the more vacant spaces, nor do nebulae appear chiefly where the stars are most clustered. It is on the borders of star-clusterings, and in the breaks of star-streams, that the nebulae show themselves, precisely as though they had taken the place of stars where star-matter began to fail.

This result is in agreement with what Sir W. Herschel observed as to the relation between nebulae and the fainter telescopic stars; for it was not in absolutely starless regions—or rather, it was not in regions *merely* starless—that he found nebulae richly spread. It was when in his sweepings he found stars beginning to fail after having been richly spread that he called to his assistant Miss Caroline Herschel, to “prepare to write, for he found himself approaching nebulous ground.”

I cannot but regard the relation here stated to exist in the maps as full of significance. After going carefully over the whole ground in mapping, I have convinced myself that the peculiarity has a real existence, that it cannot be ascribed to chance. The peculiarity recognised by Sir W. Herschel is equally striking, and we know that he was by no means disposed to regard it as merely accidental. Taking the two features, or rather the two forms of one and the same feature, into due account, it seems to me impossible to doubt that there is in these regions a real association between nebulae and stars. The nebulae here, at any rate, would seem not to be external star-systems.

If it should at any time become possible to compare the proper motions of these nebulae (some of which are gaseous) with those of the stars—either the apparent proper motions in the heavens, or motions of recess or approach—evidence of importance might be obtained respecting the relative distances of stars and nebulae in these regions. I entertain very little question as to the result of such researches. I would not venture, indeed, to assert that any marked community of motion will be

recognised when nebulae and stars are compared ; but I feel confident that the average rate of apparent nebular motion will be found to correspond with the average rate of stellar motion, and



The Nebular Region in *Coma Berenices*.

that no discrepancy will be found to exist between the *general* character of the stellar and nebular motions in the direction of the line of sight.

Graphic Conversion of Stellar Co-ordinates.

By the Rev. A. Freeman, M.A.

When a direct-vision altazimuth or a Russian diagonal instrument is to be used for observation of an object off the meridian, the object must first be found by reference to a star-map and by personal comparison of this map with the constellation in which the object appears. A celestial globe, provided with a brass meridian and a moveable vertical circle, would save time by giving at once an approximation to the altitude and azimuth of the object at the time of observation. But a celestial globe, large enough for the purpose, is both expensive and cumbersome. All that is required is a carefully constructed diagram upon which shall be drawn the hour-circles and polar-distance circles of the visible celestial hemisphere, as they are situated with reference to vertical circles and circles of equal altitude at the latitude of the observatory.

The *Nautical Almanac* gives for each day the time of transit of the first point of *Aries*, add to this the right ascension of the object, as given by its ephemeris or by a star-catalogue, and subtract the time of observation, and the result is the hour-angle of the object. Its polar-distance is also known. Hence the place of the object on the diagram is defined, and inspection will show how it is situated as regards altitude and azimuth. Thus the observing instrument may at once be set upon it.

A diagram of this kind has been drawn to exhibit the necessary circles, as they are stereographically projected from the nadir, upon the tangent-plane at the zenith of the celestial sphere in latitude $52\frac{1}{2}^{\circ}$. The horizontal circle is twenty-five inches in diameter, and successive circles of the same system differ by 5° . The latitude is that of central England. Birmingham (lat. $52^{\circ} 29'$) is the largest town which the diagram serves.* It was afterwards discovered to be equally suitable for Berlin (lat. $52^{\circ} 31'$). This intermediate latitude contributes to the clearness of the drawing, and has saved about half the labour required for the calculation of the sites of the polar-distance circles.

The *stereographic projection* has been adopted because it exhibits the whole visible hemisphere, and because all the lines required to be drawn are either circles or straight lines. This is the result of a well-known property of this projection. The angle of intersection of any two curves on the sphere is unchanged by projection. Circles are projected as circles, and all small figures are similar to their projections. Hence the four-sided figure between any two successive meridians and any two successive polar-distance circles, is projected into a four-sided figure bounded by circles, which intersect at right-angles. On this

* Norwich, Bury St. Edmunds, Peterborough, Cambridge, Leicester, Rugby, Shrewsbury, and Worcester, are each within about $15'$ of this latitude.

account it is easy to measure with accuracy the co-ordinates of a star-position falling within the figure.

The projections of the meridians, passing through two fixed points, the north and south poles, are circles having a common radical axis. The projections of the same-polar-distance circles, being circles orthogonal to the meridians, are also circles with a common radical axis, viz. the line of centres of meridians. This is easily proved by geometry.

For, if N and S (fig. 1) be the north and south poles, C and C' the centres of two meridian-circles passing through them, any point P on the radical axis, NS, is such, that the tangents drawn to the two meridians are equal, and a circle with centre P and

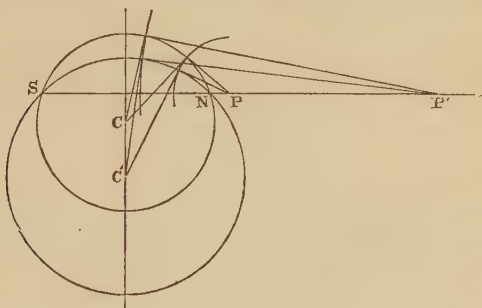


Fig. 1.

radius equal to either tangent, will cut each meridian at right angles. Also, if P' be the centre of a second orthogonal circle, the tangents drawn from C to the two orthogonal circles are each equal to the radius of the circle C; and the tangents drawn from C' are each equal to the radius of the circle C'. Hence, C C' is the radical axis of the orthogonals.

The next step is to discover mathematical expressions for the radii and centre co-ordinates of all the circles required to be drawn.

(1.) *Circles of same-zenith-distance.*—The nadir being the point of projection, and the tangent-plane at the zenith being the plane of projection, if z , measured along a vertical circle, is the zenith-distance of any point in a same-zenith-distance circle, the radius of the projected circle is $z r \tan \frac{z}{2}$; and all these circles are concentric with the zenith.

(2.) *Vertical circles, or same-azimuth circles.*—These are evidently projected into straight lines, radiating from the zenith, and the angle between a pair of successive straight lines is in the diagram 5° .

(3.) *Same-polar-distance circles.*—These circles cut the meridian at right-angles, and are bisected by it. Now, the meridian is projected into a straight line. Hence, a same-polar-distance

circle is projected into a circle cut orthogonally by the meridian, having, therefore, its centre on the meridian.

Let c be the co-latitude, and Δ the common polar-distance, then the ends of the diameter of the projected circle are at distances from the zenith equal to

$$2r \tan \frac{1}{2}(c + \Delta) \quad \text{and} \quad 2r \tan \frac{1}{2}(c - \Delta)$$

Call these d_2 and d_1 , respectively, then

$$\frac{1}{2}(d_2 - d_1) = \text{radius of the polar distance circle,}$$

$$\frac{1}{2}(d_2 + d_1) = \text{distance of its centre from the zenith.}$$

By making $\Delta = 0^\circ$, or 180° successively, we find that the projections of the north and south poles are at distances

$$2r \tan \frac{c}{2} \quad \text{and} \quad 2r \cot \frac{c}{2}$$

on each side of the zenith. The distance between the poles is consequently $4r \operatorname{cosec} c$. Hence,

$$(2r \operatorname{cosec} c - 2r \tan \frac{1}{2}c) = 2r \cot c$$

is the distance from the zenith of the line of centres of meridians, or hour-circles passing through the poles.

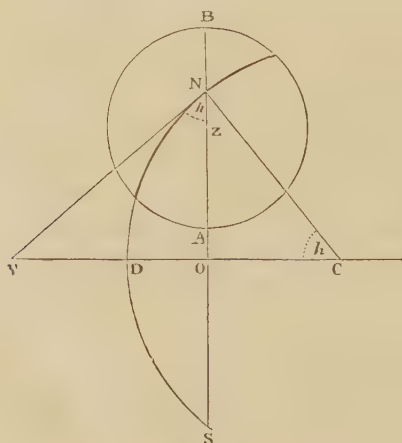


Fig. 2.

(4.) *Same-hour-angle circles, or meridians.*—Let Z, N, S, (fig. 2) be respectively the projections of the zenith, north and

south poles ; let O be the middle point of N S. Then O C, at right angles to N S, contains the centres of all the hour-circles, or meridians, through N and S. Let h be the hour-angle of the circle whose centre is C, then, by the angle property of the projection

$$\angle O C N = \angle O N T = h$$

and

$$O C = N O \cdot \cot h$$

or

$$O C = 2r \cdot \operatorname{cosec} c \cdot \cot h$$

$$= \text{distance of centre from prime meridian.}$$

It is sufficient geometrically to fix the centre C, since the required circle must pass through N. But the radius

$$N C = N O \cdot \operatorname{cosec} h = 2r \cdot \operatorname{cosec} c \cdot \operatorname{cosec} h.$$

It is convenient when the circle becomes very great to determine the position of the point D. Now,

$$D O = 2r \cdot \operatorname{cosec} c (\operatorname{cosec} h - \cot h) = 2r \cdot \operatorname{cosec} c \cdot \tan \frac{h}{2}$$

To draw such a circle practically it is best to make a template of card, with its edge forming part of a circle of the required radius ;* if, then, the edge of the template is made to rest against the points N and D, this template may be used as a ruler to draw the circle on the diagram. In the diagram constructed, this was done when O C began to exceed five feet. The length of O C in the case of the last hour-circle, 5° or 20^m distant from the meridian, would be more than 19 feet. The last four circles were practically filled in by calculating the ordinates corresponding to nine equi-distant points, at intervals of $2\frac{1}{2}$ inches along A B, and these points were joined by straight lines. These polygons are not easily distinguishable from their circumscribing circles.

The results of the calculations for the diagram constructed are these,—

$$\text{Latitude } 52\frac{1}{2}^{\circ} \text{ (Birmingham or Berlin),}$$

$$A B = 25 \text{ inches, } N Z = 4.243 \text{ inches, } S Z = 36.824 \text{ inches.}$$

Hence,

$$N S = 41.067 \text{ inches, } N O = 20.5335 \text{ inches, } Z O = 16.290 \text{ inches.}$$

Radii of same-zenith-distance circles.

	Radius.	z	Radius.	z	Radius.
5	0.545	35	3.941	65	7.963
10	1.094	40	4.550	70	8.753
15	1.646	45	5.178	75	9.592
20	2.204	50	5.829	80	10.489
25	2.771	55	6.507	85	11.454
30	3.349	60	7.217	90	12.500

* Suggested by Prof. Cayley.

Radius, and distance (north of zenith) of the centre of each same-polar-distance circle.

Δ	Centre-Dist.	Radius.	Δ	Centre-Dist.	Radius.
5	4°252	0°609	70	6°702	10°345
10	4°279	1°221	75	7°232	11°476
15	4°325	1°839	80	7°869	12°730
20	4°391	2°467	85	8°642	14°142
25	4°477	3°108	90	9°592	15°756
30	4°592	3°760	95	10°775	17°633
35	4°719	4°446	100	12°279	19°864
40	4°880	5°152	105	14°236	22°588
45	5°065	5°897	110	16°860	26°025
50	5°299	6°667	115	20°525	30°558
55	5°567	7°491	120	25°877	36°902
60	5°883	8°370	125	34°624	46°590
65	6°258	9°342			

Perpendicular distances from the meridian of the centres of successive hour-circles.

h	Distance.	h	Distance.	h	Distance.
h m		h m		h m	
0 20	234°699	2 20	29°325	4 20	9°575
0 40	116°451	2 40	24°471	4 40	7°371
1 0	76°632	3 0	20°534	5 0	5°502
1 20	56°415	3 20	17°230	5 20	3°621
1 40	44°034	3 40	14°378	5 40	1°796
2 0	35°565	4 0	11°855	6 0	0°000

To test the accuracy of the diagram actually constructed, it may be noticed that four circles seem to meet in one point where

$$h = 60^\circ, \quad \Delta = 75^\circ, \quad z = 60^\circ, \quad A = 75^\circ.$$

Calculation proves the true corresponding values to be

$$z = 60^\circ 2'6, \quad A = 74^\circ 54'6.$$

Again, four circles seem to meet near a point where

$$h = 70^\circ, \quad \Delta = 45^\circ, \quad z = 45^\circ, \quad A = 110^\circ.$$

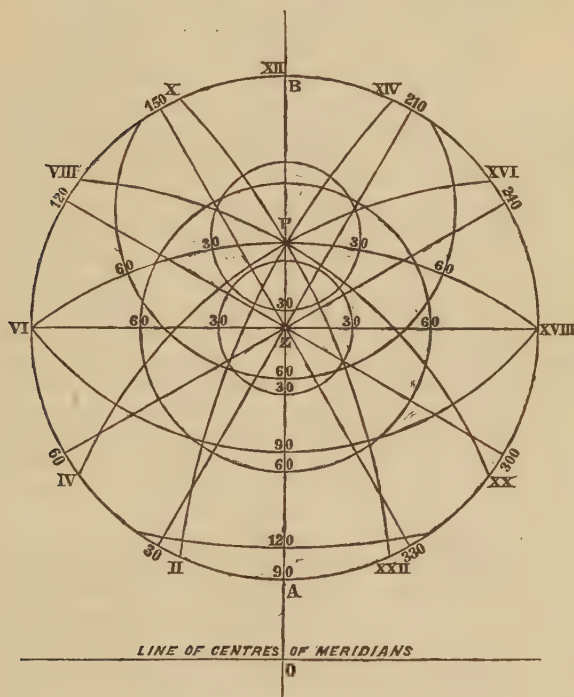
Calculation gives,

$$z = 44^\circ 54'6, \quad A = 109^\circ 45'1.$$

Upon the diagram there are about 650 intersections of polar-distance circles with hour-circles *east* of the meridian. The practical observer might, perhaps, find it best to measure on his diagram, once for all, the corresponding zenith-distances and azi-

muth, and tabulate them for reference. The table would then supersede the diagram.

Stereographic projection for the conversion of polar-distance and hour-angle into zenith-distance and azimuth.



(Fig. 3). Lat. $52\frac{1}{2}^\circ$.

If it were considered advisable, diagrams such as the present could easily be constructed, for use with the diagonal altazimuths at each of the five English stations for the observation of the transit of *Venus* in 1874.

An abridgment of the diagram for latitude $52\frac{1}{2}^\circ$ is shown in fig. 3.

St. John's College, Cambridge,
October, 1872.

On the Examination of the Photographs taken during the Total Solar Eclipse of December 11-12, 1871, at Dodabetta. By Lieut.-Col. J. F. Tennant, R.E., F.R.S.

In the text of my report on the eclipse, I have expressed my opinion that the relation between some of the prominences and

neighbouring features of the corona is so marked that it points to an evident connexion. A careful examination quite confirms this; and I believe any one who has access to the photographs will be led to the conclusion that the relative positions of the traces of the prominences and of the coronal features are unchanged, and that the Moon, which we know to move with respect to the former, does so equally with respect to the latter. It has, however, been thought most desirable that this general impression should be confirmed, and I have now to describe the measures which have been taken to this end; in doing so I would, however, wish to say, that Dr. De La Rue has aided me every way with apparatus, advice, and time; and that, indeed, without him the data for this memorandum would not have existed.

In the first place, and as a preliminary to other steps, the negative photographs were placed in an oxy-hydrogen microscope, and projected on to mounted drawing boards, on which the main features were sketched. The enlargement of the Moon's diameter (about 24·5 times) gave it about 7·64 inches, and care was taken to keep this constant by retaining the adjustments. Careful tracings were made of the sketches, and an attempt was made to determine roughly the amount of the lunar motion with respect to the corona. Two sketches had been made of the projection of No. 1 photograph; one, mainly by Dr. De La Rue, is spoken of as No. 1, while the other, mainly by myself, is distinguished as No. 1 T. In the following process the results from these furnish some check on each other:—

A tracing, say of No. 2, was laid on sketch No. 1, and the greatest coincidence of the general features of the corona produced by shifting the tracing on the sketch, each tracing and sketch having had marked on it a circle which generally coincided with the lunar limb shown on it, and the distance of the centres of the circles was assumed as the apparent motion of the Moon with respect to the corona, as deduced from those two pictures. In the first place, it was found that the lunar motion so deduced was always in the right direction, or towards the following side, and slightly to the south. To come to details, comparing

No. 1 with No. 2 the displacement was 0·055 inch.

„	No. 3	„	0·110
„	No. 4	„	0·180
„	No. 5	„	0·195

In dealing with these numbers it became necessary to assume an epoch for each picture. Dr. De La Rue thinks that the instant of opening the lens should be the assumed epoch, but I propose to use the middle of the exposure, which seems to me more satisfactory; and I have ascertained that the difference will not be important. From the moments of exposure and closing the lens, given on page 11 of my report, I find the most probable value of

the motion of the Moon in one minute to be 0.150 in. on the scale of the tracings.

Again, comparing No. 1 T in the same way with each of the same tracings, we have

No. 1 T compared with No. 2 gives for the motion 0.110 inch.

"	No. 3	"	0.125
"	No. 4	"	0.210
"	No. 5	"	0.250

It is quite evident that there is a considerable difference shown by these figures in the estimated centres of No. 1 and No. 1 T; considerable, that is, with respect to the quantity to be determined. The mean amount seems to be 0.035 in., and if this quantity be deducted from all the motions formed by comparisons with No. 1 T, we have a probable motion from these comparisons of 0.1485 in. per minute. The motion which I have derived from computation is 0.107 in., and it will be evident that the error is not more than might be anticipated from the process; while, at the same time, it appears clear that there is an actual motion of the Moon with respect to the general corona, just such as exists with respect to the prominences and Sun.

In the course of these comparisons it became evident that the resemblance between the photographs was no general one, but that every ray, or flame, and rift, could be traced through the whole of the photographs employed in them, though considerably changed sometimes by the varying circumstances of the photograph. This circumstance rendered more probable success in measuring the details of the corona with Dr. De La Rue's micrometer, which had been proposed.

Accordingly a catalogue of objects was made, and No. 1 Photograph having been placed on the micrometer-table, and duly centred, 26 position-angles of rays, and salient angles, were measured with corresponding distances, and ten angles and distances of the bottoms of rifts. Where there was an appearance of two or more envelopes, separate observations were made, but the set is counted as one. Photograph No. 3 was then chosen for comparison, and placed on the table, and measurements were taken of the corresponding points, which were without difficulty identified. One remarkable difference was noted. In No. 1 there is a remarkably long ray, with a position angle of 99° , which projects faintly as much as $46'$ from the lunar limb. In No. 3 this was much restricted, and very difficult to distinguish from a neighbour which, in No. 1, was not two-thirds of the length. This, however, must not be considered evidence of change, as every ray in No. 3 is less extensive than in No. 1, and this long faint ray may well have suffered more than its more marked neighbour, from the cause which has shortened all the rays.

The greater part of these observations was made by Dr.

De La Rue, and verified by myself. A commencement was made by the reverse procedure, but the greater delicacy of Dr. De La Rue's eye soon became evident, and he very kindly gave the time necessary. Under these circumstances, I am, perhaps, hardly able to judge of the value of the measurements; but as far as it is worth anything, my opinion is, that while entirely confirming the statement of the general permanence of the corona, these measurements do no more. The objects to be observed are very ill defined, and towards the limits of the silver deposit forming the photograph it is sometimes impossible to see even traces of it, at the same time as the wires of the micrometer. A reference to the neighbouring specks on the film is, in these cases, inevitable, and it must be difficult in the extreme to avoid noting mentally the position of the estimated points of a ray as compared with these definite marks, and once this is done I cannot feel that accordances give me any evidence of value. I propose therefore resting the evidence of lunar motion on the evidence of the previous examination, and omit the figures representing the positions determined.

Before closing, I may mention a further experiment. One edge of a rift, that which I have spoken of in page 7 of my report as nearly vertical, and which was the first across which I laid the slit, was so well marked that it seemed practicable to determine if this edge moved with respect to the Moon's centre, and its position made the direction of the lunar motion not far from perpendicular to the line. I, therefore, determined the co-ordinates of a point on the estimated boundary with reference to axes passing through the lunar centre, one of which was parallel and the other perpendicular to the direction of motion. If there were no change in the corona the latter ordinate would alone be altered, and in examining No. 3, the ordinate in a direction parallel to the lunar motion was measured for the same value of the perpendicular ordinate. This showed an apparent motion of $0\cdot046$ in., (the lunar diameter being $0\cdot312$ in.), an enormous amount plainly ascribable to reduced effect of the light in the second picture; and it shows how unsafe a comparison, involving an edge, is. But when the wires were placed in the same position relatively to the Moon's centre, as in No. 1, it became quite evident that no increased action of the light could have placed the side of the rift in such a position, and that there was real motion.

On the whole, I think there can be no question that there is a real motion of the Moon across the generally constant phenomenon of the corona, and this being admitted, I anticipate that the conclusion will be generally accepted, that the portions of the corona shown on the Dodabetta photographs are solar. Such being, I believe, the deduction from the evidence they alone supply, the fact that they very closely resemble the pictures taken by Lord Lindsay's assistant at Bekul, strengthens the conclusion, and shows that the changes are not sensible even in longer intervals than those between my extreme pictures.

Probably many will think I might have relied more on our measurements, but I shall be content if they accept my conclusions; and I trust that the Royal Astronomical Society will be able to give, with my papers, such engravings of four of the photographs as will show the kind of agreement which exists.

Deal, August 24, 1872.

On the Arc of the Meridian Measured in South Africa.

By I. Todhunter, M.A., F.R.S.

1. It is well known that in 1752 La Caille measured an arc of the meridian in South Africa, and that the result obtained was decidedly larger than might have been expected from the measurements in the Northern hemisphere. It is also well known that Sir Thomas Maclear, formerly Astronomer Royal at the Cape of Good Hope, has in recent times carried on another measurement in South Africa, far more extensive than La Caille's, and that the result of the new operation is in harmony with the average of the trustworthy Northern arcs. Various references to these recent operations will be found in the *Memoirs* and *Monthly Notices* of the Astronomical Society; but I think no account has been given in these publications of the two large volumes issued in 1866 containing the details of the astronomical and geodetical work. There can be but one opinion of the value of Sir Thomas Maclear's measurement, and of the zeal and energy with which it was conducted. The Royal Society has marked its approbation by conferring a medal on the eminent astronomer; see the *Proceedings of the Royal Society*, vol. xviii. p. 109. The Lalande medal of the Academy of Sciences at Paris has also been awarded to Sir Thomas Maclear for this work.

2. The title of the volumes to which I have alluded is *Verification and Extension of La Caille's Arc of Meridian at the Cape of Good Hope*, by Sir Thomas Maclear, Astronomer Royal at the Cape of Good Hope. These volumes were published in England, under the editorship of the Astronomer Royal.

3. In consulting these volumes for the purpose of a *History of the Theories of Attraction and the Figure of the Earth*, it has appeared to me that there are some points in which explanations and additions are required; and I hope it may not be impossible to have a supplement of a few pages, for the sake of giving completeness to the publication. There must be at present various persons who are qualified to throw light on the subject; and thus it may happen that information could now be supplied which in a few years will be quite unattainable.

4. As the volumes now stand there is a very great discrepancy between the title and the contents. Instead of having "a verification and extension of La Caille's arc," we have the account of a new arc; and there is no comparison whatever of La Caille's *length* with the new *length*, though there is a comparison of La Caille's

amplitude with the corresponding part of the new *amplitude*. Thus when we read on the title-page of vol. i. as part of the contents, *Comparison of the Ancient and Modern Measures*, we are naturally led to expect what we do not find. There are apparently indications in the book that such a comparison was to be made; see pages 232, 403, 452, and 456. (All the references I shall have to make will be to the *first* volume.) There are also apparently allusions to some discussion of the disturbances produced by the attraction of Table Mountain; see pages 3 and 83, but no such discussion is given. We also read in the advertisement by the editor that "the publication of the work has been delayed about two years in the belief that it was the intention of Sir Thomas Maclear to add another sheet." And although we are afterwards told that "no addition is to be made," and that "the work is complete in itself," yet I venture to hope that something more will be soon supplied.

5. The amplitude of La Caille's arc was verified, as I have stated. The modern result does not differ from La Caille's by so much as half a second. We read on page 111 " . . . It redounds to the credit of that justly distinguished astronomer, that with his means, and in his day, his result from sixteen stars is almost identical with that from 1133 observations on forty stars made with a powerful and celebrated instrument." This is certainly a very remarkable confirmation of La Caille's accuracy; it must, however, be remembered that there is some uncertainty as to the exact spot which formed La Caille's northern station; this remark must be borne in mind throughout, though we shall not repeat it. La Caille had always enjoyed a great reputation for accuracy; see, for instance, the *Base du Système Métrique*, vol. iii. pages 162 and 544; and the article on the "Figure of the Earth" in the *Encyclopedia Metropolitana*, page 207. The confirmation of La Caille's value of his amplitude naturally increases the desire to compare his length with those of the recent survey.

6. According to page 219 of the article on the "Figure of the Earth" just cited, La Caille's arc is 1051 feet longer than corresponds to the average of the arcs in the Northern hemisphere. Taking the amplitude at $1^{\circ} 13' 17''$, this is at the rate of about 860 feet for a degree.

7. A letter written at the Cape of Good Hope by Sir Thomas Maclear, dated the 6th of May, 1846, and addressed to Schumacher, is published in the *Astronomische Nachrichten*, Number 574, September 3rd, 1846; the number forms part of vol. xxv. Here we find some of the comparisons which we require. The lengths of the five principal lines in La Caille's triangles are given, according to La Caille's own numbers, and also as deduced from the modern base with La Caille's triangles. According to this it seems that La Caille's numbers are about $\frac{1}{30000}$ th part too large. It is stated that these differences would disappear by subtracting about fourteen feet from La Caille's base. La Caille's

base, however, seems to have been measured with great care; see pages 432, 436 of his memoir in the Paris *Mémoires* for 1751. The lengths of three of the lines are also given as calculated from the modern base with the modern triangles; these scarcely differ from those calculated from the modern base with La Caille's triangles: hence I presume we may infer that La Caille's geodetical angles were sufficiently accurate.

8. The letter contains another very important table, from which I extract the following, which relates to the distance between Cape Town and Klyp Fontein,—

Amplitude.	Length.	Length of 1°.
0 " "		
I 13 17'33	445506 feet	364728·8 feet
I 13 17'33	445361	364607·5
I 13 14'51	445027·51	364568·3

The first is said to be La Caille's result; the second, the result with La Caille's triangles and the modern base; and the third, the result with the modern geodetical and astronomical measurements. The lengths are given in English feet, except La Caille's arc; this is put at 69669·1 toises, which I have turned into feet. It ought to be observed that La Caille's toise has been held to have been somewhat too long, in consequence of which his degree was three toises too short; see Lalande's *Astronomie*, 3rd edition, Art. 2649. Then we read, "There is no necessity for examining at present why La Caille's measurement of the degree between Cape Town and Klyp Fontein should exceed the modern measure of the same by 160 feet. That inquiry may be worth consideration when the present work shall have been rigorously reduced." Thus it would appear that the modern measurement effected a reduction of only 160 feet in the length of the degree as derived from La Caille's original arc. But there is here a serious difficulty; for the modern amplitude is now taken as nearly *three* seconds less than La Caille's, whereas originally these amplitudes were made to agree within less than half a second; see Art. 5. I do not understand this contradiction.

9. The letter in the *Astronomische Nachrichten* contains some other interesting points which may be noticed. It is asserted decidedly that "no use can be made of La Caille's arc, properly so called, in the question of 'The Figure of the Earth.'" Again, "The chief cause of the failure of the measurement of 1752 rests with the circumstances of the terminal points." Then it is stated that Table Mountain to the south of La Caille's southern station, and high mountains to the north of the northern station, exerted an influence: "Consequently the arc is shortened by the sum of the deflections at its termini." It is stated that "Klyp Fontein station is close up in the south-west angle of large mountains;" but this is not quite clear, because we do not know the point of reference or origin from which we are to proceed to the south-west.

10. With respect to the disturbances and to the attraction of mountains it is uncertain whether the opinion of Sir Thomas Maclear remained permanent. In the letter, as we have seen, he attributes the anomaly entirely to this; but in the volumes I think he never decidedly takes the same view, though he certainly mentions mountains to the north of La Caille's northern station; see pages 39 and 403. But judging from La Caille's map and from Sir Thomas Maclear's Plate V., the preponderating action at the north end does not seem to be obviously and decidedly towards the north; the mountains extend not indeed directly *south* of the station, but still south of the latitude of the station, as well as north of the station. Again, as to Sir Thomas Maclear's *southern* station, we read in the letter, "Zwart Kop is about eight miles on the meridian north of Cape Point, and it is adopted as a southern terminus of the arc in preference to Cape Point, because Cape Point is the termination of a rugged peninsular range of mountains, without any solid matter above water to the south, and is therefore objectionable as the end of an arc of meridian, although interesting as affording the materials for a philosophical experiment." It is uncertain whether the opinion of Sir Thomas Maclear on this point remained permanent; but judging from the volume it would seem that Cape Point is really the better terminus; see page 609.

11. Now let us turn to the *Monthly Notices* of the Astronomical Society, vol. vii. 1847, page 58. Here we read, ' . . . Mr. Maclear (1840-41) carefully measured a base of 42,000 feet, nearly on the site of La Caille's, and then reobserved all his triangles, feeling confident that the former stations had been recovered in every instance to within a few feet. The length of the degree thus found halved the difference between La Caille and the modern state of the theory, being about 200 feet less than that of the former, and about as much more than that of the latter.' There are difficulties here. First, we have 200 feet assigned for the excess of La Caille's degree above that recently obtained, whereas it is elsewhere put at 160 feet; see Art 8. Secondly, we have the error that the whole difference between La Caille's degree and that of modern theory is about 400 feet; whereas we know it is about 860 feet; see Art. 6. There is another error on the same page of the *Monthly Notices*; it is asserted that the modern arc is of about $4\frac{1}{2}$ degrees, terminating at Khamies-Berg station; but the fact is, that the arc up to this station would be of about four degrees, and the additional half degree was obtained by proceeding beyond Khamies-Berg.

12. Next we take the *Monthly Notices*, vol. xviii. 1857, pages 313-16. Here we have a very important letter from Sir Thomas Maclear giving an account of his operations. We read, at page 315, that "the distance between Bradley's Sector-station at Klyp Fontein and the Rogge Berg Sector-station is, by the modern triangulation, 445018.14 feet." I assume that Rogge Berg is a misprint for Rogge Bay. Then to find the length of

La Caille's arc we must allow for the slight difference between his end points and those here named. According to the *Verification and Extension*, page 111, we have to add $206 + 45$ feet on this account, and so we obtain 445279.14 feet for the modern value of La Caille's arc. The difference between this and La Caille's own result is about 235 feet. It may perhaps be inferred that when all the geodetical work for La Caille's arc is rendered perfect, there remains still an excess of about 800 feet. This for want of any other explanation we may attribute to disturbance of his zeniths by local attraction; it corresponds to about eight seconds as the sum of the deviations produced at both ends. We may observe that Sir Thomas Maclear's own arc would have apparently suffered about eight seconds of error if he had not prolonged it through the last half degree; see his page 609.

13. There is a difficulty on page 314 of the volume of the *Monthly Notices* last quoted. The height of Heerenlogements Berg is stated in one place to be 2381 feet, and in another 1939 feet; perhaps in the second place we ought to substitute Zwart Kop.

14. Sir Thomas Maclear devoted great attention to the investigation of the precise points which La Caille took for his extreme stations; there are, however, some matters which do not appear quite clear. We begin with the situation of La Caille's observatory in Cape Town.

15. We read on page 9, " . . . carries us . . . to the point marked on the plan as the probable site of the Observatory." Perhaps by the word *point* here is meant the *shaded rectangle* in Plate I.; or perhaps *observatory* is used instead of *sector*. If the shaded rectangle be intended for the observatory, the position does not seem to correspond with La Caille's statement that it was "at the *bottom* or *further end* of the court;" see page 7.

16. We read on page 9, "The meridian of his observatory passes to the east of Mrs. De Witt's house, a couple of degrees to the left of the high projection into the yard. . . ." And in a note, "The meridian of his sector, I believe, passed over the high buildings." It is not obvious what is here meant by "a couple of degrees," as we are not told where the centre of the circle is placed on which the degrees are taken. Then we are not told how it is certainly known that such was the position of the meridian of La Caille's observatory. The *high projection* appears to be different from the *high building of two stories* which is mentioned in the next page. The "meridian of the sector," which occurs in the note is, we may presume, not identical with the "meridian of the observatory," which occurs in the text. Finally, the "high building" of the note is perhaps identical with the "high projection" of the text, and a smaller structure than the "high building of two stories;" in fact, from the size of the last, there would be nothing definite in saying that the meridian passed over it.

17. We read on pages 9 and 10, "From the north-west wall

of the kitchen within the yard projects the pin, or spike, alluded to by Captain Everest, flattened at the end, where it is slightly turned downwards. In the flat portion there is a hole, and underneath, on the wall, is drawn a rectangular quadrilateral, bisected perpendicularly. A pencil of light through the hole falls on the middle vertical line at apparent noon. Captain Everest speaks of "a brass plate perforated with a small hole, and fixed horizontally in a vertical wall. . . ." *Memoirs of the Astronomical Society*, vol. i. page 262. This seems to be the only relic of La Caille's residence at his Cape Town Observatory; but it is difficult to imagine that the same object can be described as "a pin, or spike," and also as a "brass plate." In a note on page 10, we read, "This pin is in the meridian of the observatory" It is not obvious how the meridian of the observatory was known independently, so as to justify this statement. We might perhaps have expected such words as these, "It is natural to assume that the meridian of the observatory must have passed through this pin." In Plate I., the line which is apparently intended for the meridian of the observatory does not pass through the position assigned for the pin.

18. On page 10, we are told of "a high building of two stories," and also of "low buildings," which had been erected since La Caille's time. Then it is added, "The upper story of the high buildings is let to sail-makers, and the lower buildings to fishermen. The latter occupy the site of the observatory." The editor has found this passage difficult; he conjectures that *low buildings* should be changed to *lower story*, and he says in a note, that "the high buildings occupy the site of the observatory." On page 62, we read, "The upper story forms the sleeping-room of Mr. Vansittart, a relative of Mrs. De Witt, and the lower is a receptacle for household stores." This statement may perhaps refer to a year later than the former; but we see that if we adopt the suggested correction of *low buildings* to *lower story* in the former, then we have the use of both upper and lower story changed in the interval. Some further notice of the locality is given on page 412.

19. We will now advert to La Caille's northern station. On page 50, there is a table with respect to the points near this station. Here the azimuths of La Caille's north and south peak do not differ by so much as on page 42. Perhaps this may be the matter to which the note on page 50 refers.

20. On page 41, we read, "I soon found that I must distinguish the several masses from each other by names to prevent confusion in the entries." Then followed the names, among which we read, "The mountains close to the sector, La Caille's Mount." We find elsewhere, as on page 44, the names "La Caille's Foot," and "La Caille's Head." On Plate IV. there are two views and a profile. *La Caille's Mount* is not mentioned, though *La Caille's Head* and *La Caille's Foot* are. It does not seem certain what is really meant by *La Caille's Mount*.

21. The second view on Plate IV. is said to be "from 200 yards beyond the north end of the base line." It is not clear whether the 200 yards are to be taken in the direction of the base line produced. The profile is said to be "from north-west to south . . . looking east." This we may presume means "turning from north-west to south through the east;" but it is not certain. Plates V. and VI. do not agree as to the position of the south peak of La Caille's Mount. In Plate V. this peak is close to the middle peak, but in Plate VI. the two are much separated.

22. It would be convenient to have a distinct account of the numerous peaks to which La Caille's name is given, in order to be certain of identifying them again if necessary. The *Head* has apparently three peaks, and the *Foot* two, according to Plate V.; but the views on Plate IV. do not seem to indicate these very distinctly.

23. On page 403 it is said, that "the northern extremity is close up in a corner formed by a north-westerly bend of Picquet-Berg." The words *close up in a corner* do not seem to correspond with Plate V.; unless by *in a corner* we understand *outside* a corner.

24. The position of La Caille's northern station may perhaps have been fully made out; but this is a point on which naturally those who have examined the locality are entitled to decide; but there appear some difficulties to those who compare the accounts. La Caille's map is probably not to be treated as extremely accurate; but it does not quite agree in its figure of the mountains with Sir Thomas Maclear's plates. La Caille, in his *Journal*, pages 181 and 182, says that a wide prospect could be seen from his station, even Table Mountain, for example; in fact, almost all that could be seen from the summit of Picquet-Berg. And yet he speaks of the habitation called Klyp Fontein being at the *foot* of a mountain, *adossée au Picquet-Berg*. It is quite obvious that these statements completely apply to the site as fixed by Sir Thomas Maclear.

25. Various notices respecting the South African operations will be found by the aid of the Indexes to the *Monthly Notices* and the *Memoirs of the Astronomical Society*; but I do not think that there are any matters of importance in them to which I have not alluded except what will be found in two papers which I will now mention. One of these papers is by Captain Clarke in the *Monthly Notices*, vol. xix. pages 36-38; here the effect of the new arc in modifying the best results previously obtained is investigated. The other paper is a memoir in the *Memoirs of the Astronomical Society*, vol. xx. 1851, entitled "Report of Proceedings at the Royal Observatory, Cape of Good Hope." On pages 7-25, we have an account of the operations connected with the arc of the meridian from the autumn of 1842 to 1848; this resembles pages 402-418 of the *Verification and Extension*, but is not identically the same; both accounts are very interesting and

should be read. Here we find on page 7 "La Caille's measure was vitiated, and rendered useless as an element in the determination of the figure of the Earth, because his terminal points are affected by the sum of the attraction of Table Mountain and the northern end of Picquet-Berg." As I have already stated in Art. 10, there seems no such decided opinion recorded in the *Verification and Extension*.

26. In conclusion, I may say that it is much to be wished the volumes had been furnished with an index, or at least with a copious table of contents. Moreover, there are various places in which the editor in notes points out difficulties in the text and sometimes suggests corrections; it would be useful to know if in these cases any explanations can be furnished, or whether Sir Thomas Maclear himself always adopted the suggestions.

St. John's College, Cambridge,
October 8, 1872.

On Lord Lindsay's Preparations for Observations of the Transit of Venus, 1874. By Lord Lindsay and Mr. David Gill.

An account of any preparations for the observation of the Transit of *Venus* in 1874, will doubtless be of some interest to the Fellows of the Society, and it is partly with this in view, but especially with the hope of securing more perfect co-operation with other expeditions, that this preliminary account of our preparations has been drawn up.

The station selected is the Island of the Mauritius. Its very favourable meteorological conditions offer every hope of fine weather during the transit, whilst its geographical position is very favourable for the observation of retarded ingress, and sufficiently favourable for the heliometric method, *i.e.*, for the displacement in line of centres. The station having been thus selected we have for method of observation choice of,

1st. Observation of internal contact for application of the methods of Halley and Delisle.

2nd. Observation of external contacts by viewing the approach of the planet to the limb through the chromosphere, the light of the sun being dispersed by a powerful spectroscope.* (These observations are of course equally applicable, with co-operation at a northern station, to Halley's or Delisle's methods.)

3rd. The Photographic method.

4th. The Heliometric method.

There is something to be said in favour of each of these methods, and as it was found that two observers with efficient assistance, and good organisation could accomplish the whole it was determined to provide for them all.

We shall deal with them in succession.

* For a description of the method I should use for this purpose I refer the Fellows to a note of Padre Secchi to Professor Silliman, and which is to be found in *Sill. Journal*, Third Series, Vol. I., No. 6 (June, 1871), page 463.

First. Observation of the true instant of contact (to apply both Halley's and Delisle's methods) involves an exact knowledge of the latitude, longitude, and local sidereal time.

We are not yet certain what facilities exist for the transport of chronometers from Alexandria (whose longitude will probably be determined before 1874 by telegraphic signals); in the event of no satisfactory determination of this kind being possible, we intend to rely on observation of the Moon's place by occultations of stars, transits of the Moon, as well as observations of her altitude and azimuth.

For the latter method we shall take out an altazimuth just completed for Lord Lindsay by Troughton and Simms, and which is now under trial at Dunecht, with circles of 12-inches in diameter divided to 5' and reading by two opposite microscopes to single seconds of arc.

On account of the low latitude of the Mauritius the method of azimuths will only be applicable when the Moon has considerable north declination and is very near the meridian. The instrument is therefore provided with horizontal webs for vertical transits.

We believe Germany also intends to send an expedition to the Mauritius, which will also probably be provided with the means of determining its geographical position, so that connecting the two stations by triangulation a combination of the observations of both parties will give more quickly an accurate result.

The transit Lord Lindsay hopes to make the subject of a separate communication, and to exhibit at a future meeting; we need only say at present that it is now being made by Messrs. Cooke, of York, and is of 4 inches aperture.

The clock is a very excellent one by the late Charles Frodsham, with dead-beat escapement and mercurial pendulum, and is fitted with the necessary connexions for registering its beats on the chronograph boards.

The chronograph will also be described more minutely afterwards. It is now being made by Messrs. Cooke, for the Observatory, at Dunecht. There are four barrels, any one or all of which can be put into action, or detached at pleasure; these barrels are 2 feet long, and 1 foot in diameter.

In the Observatory each barrel revolves once a minute, and contains records for two hours. For the special work of the transit the speed of revolution is reduced to one half of that amount by a change-wheel, so that each barrel can record four hours' work, or the work of the whole duration of the transit.

The advantage of four barrels is that each instrument will have its own barrel, and there can thus be no confusion of the records.

Lord Lindsay will take up the observation (by the spectroscopic method) of the external contact, if co-operation at a northern station can be secured; and either internal contact by the same method, or by the ordinary method of eye-observation with a refractor of 6-inches aperture, as may be hereafter determined.

Mr. Gill will observe internal contact with a refractor also of 6-inches aperture equatoreally mounted. Records in all cases will be made by eye and ear as well as by the chronograph.

The sun shade, magnifying power, &c., to be employed should be made the subject of arrangement, so that in all expeditions, where the same aperture of telescope is used, the conditions of observation should be made otherwise as identical as possible.

3. We now come to the Photographic Methods.

In order to obtain the necessary accuracy it seems to be agreed on all hands that an image of the Sun of at least 4 inches in diameter is required.

This granted, it appears to us the most essential element of success that this image should be of the highest possible perfection; and as pointed out by Lord Lindsay and Mr. Ranyard (*Monthly Notices*, 14th June, 1872) should be formed by rays as little oblique as possible.

These conditions appear to be best fulfilled by Professor Winlock's method of using a telescope of about 40 feet focus; the Sun's rays being reflected into it horizontally by a Heliostat, and forming in the primary focus an image of the Sun of rather more than four inches in diameter. This method appeared to Lord Lindsay to possess so many advantages that he long ago ordered from Mr. Dallmeyer a simple lens like Professor Winlock's of 4 inches aperture, and 40 feet focus; but afterwards argued that if such perfect pictures could be obtained by an uncorrected lens of this focus, how much more perfect pictures could be obtained with a compound lens properly corrected for the chemical rays, and for spherical aberration.

Mr. Dallmeyer has undertaken to furnish such an object-glass for experiment by the end of this year.

There is no doubt that such a lens will give a picture which may be considered absolutely perfect, provided we can ensure the optical perfection of the plane of the Heliostat. It was at first intended to employ two planes, one mounted on the lower end of a polar axis which rotated so as to reflect the Sun's rays in the constant direction of the pole, and to receive these on another plane to make the direction of the beam horizontal. At this time Lord Lindsay had seen no Heliostat which he considered sufficiently perfect, until in February last when visiting the Imperial Observatory at Paris, he saw the Great Siderostat of Foucault there, and at once recognised in it the instrument he required.

For further particulars of this beautiful instrument I must refer the Fellows of the Society to Mons. Wolf's description of it in *Les Annales Scientifique de l'Ecole Normale Supérieure*, 2nd Serie, Tome I. It will suffice to say here that by means of two axes it causes a mirror of 11·8-inches diameter to turn so as to reflect the rays of the Sun in a constant direction horizontally; and it is also provided with the means of giving slow motion in R.A. or Decl., at the pleasure of the observer, by handles which can be of any length.

Mons. Eichens undertook to complete such a Siderostat for this expedition in the necessary time with two plane mirrors of 16 inches in diameter, by Mons. Martin, to be used in connexion with the 40-foot telescope.

It may appear that 16 inches is quite an unnecessary diameter for the purpose required. Undoubtedly it is, but we intend to apply the Siderostat to many after purposes (such as the observation of the Sun with larger and heavier spectroscopes than could be conveniently attached to an equatoreal.) Nevertheless, if we have a plane of this large size, and as perfect as that seen by Lord Lindsay on the Siderostat of the Paris Observatory, giving very excellent definition at very oblique angles of incidence, have we not in this very large plane the still greater probability of getting a limited number of square inches in its centre almost entirely faultless? So much, however, has been said on the possible evil effects of interposed planes, and of their distortion by heat* that we should not consider any preparation at this stage complete without some effort to attain the required perfection of picture without their aid.

Seeing that an absolutely perfect enlarging eye-piece has not yet been obtained, though believed to be now nearly accomplished, and seeing that all such form the picture by very oblique rays, we had to fall back on the Reflector.

A Cassegrain Reflector of 10 feet focus can readily be made to form an image of the Sun of 4 inches diameter, and of very great perfection. Mr. Howard Grubb has accordingly undertaken to make the mirrors of such an instrument to adapt into the lattice tube of a 13-inch silver on glass Newtonian which we take for other purposes with us. The principal mirror of this will also be of 13-inches diameter, and it is hoped that the best results will be obtained with both great and small mirrors unsilvered.

Special adaptations for getting rid of the transmitted heat, for adaptation of the camera and discharge of the exposing shutter, are being carried out; but an account of these is deferred for a future communication. The instants of exposure will in both cases be automatically recorded on the chronograph barrel, as well approximately, for check, by eye and ear.

With reference to the first of these two photographic methods, it is obvious that the usual mode of reference to fiducial lines is not available, as the image of the Sun will revolve about its centre, in other words, the zero of position is constantly changing.

It is, of course, possible to ascertain by calculation the position of a fixed line relative to any of the pictures of the Sun, provided that its position at a known time, the instant of present exposure, and the errors of adjustment of the Siderostat, are known.

* In M. Wolf's description of the Siderostat referred to above, occurs the following important passage bearing on this point: "J'ajouterai ici un seul fait, d'une importance capitale: exposé pendant une heure aux rayons d'un soleil d'été, avant l'argenture, le miror a conservé sa surface optiquement plane."

The question remains, *Is it desirable to have any fiducial line at all? is it not probably better to treat our photographic pictures as we should heliometric measures?* Measuring in each picture only the true distance of the planet from the centre of the Sun, and knowing the instants of exposure, the relative angular motion of *Venus* and the Sun, measures of each pair of sufficiently separated pictures will give one of a series of expressions for the chord described by the planet or the least distance of the centres of the planet and the Sun.

Such treatment of the pictures renders us entirely independent of any fiducial lines whatever, and we cannot help thinking that such lines will be apt to lead into error. We question very much whether the direction of fiducial lines can be determined with extreme accuracy on such instruments as the Kew Photo-heliograph. It must be remembered that the zero of position angle will vary very considerably if the equatorial adjustments are not very good, or at least not carefully ascertained and allowed for, and we must also remember the adjustments of any metallic telescope mounting are very seriously disturbed by exposure to the Sun. On this account we think that the measurement of position angles should be entirely dispensed with.

Contraction of the film can be eliminated, but this must also form the subject of another communication, as also the best method of photographic procedure and manipulation, and the best form of micrometer for the measurement of the pictures.

4. Lastly we come to the Heliometric Method.

This method has not found favour in this country,—at least, so far as we know, none of our Government expeditions will be equipped with heliometers. In Russia and Germany much reliance is placed on this method, and we think with good reason.

It is true that the geographical position of the Mauritius is not very favourable for this method of observation, the co-efficient of parallax for least distance of centres being comparatively small—only $\frac{3}{10}$ ths of the greatest possible.

On this account the German Government, we believe, intends to send a heliometer to the more favourable station of the Kerguelen Island, or the Macdonald Islands, and one to the Auckland Islands, but as the meteorological conditions of these islands are so uncertain they have also selected the Mauritius as a third heliometric station. For corresponding observations in the northern hemisphere, a German expedition is proposed to be sent to the North of China (Chefoo probably), leaving the other northern observations to the Russian expeditions.

We believe the Russian Committee intends to send a heliometric expedition to Lake Baikal, and to the mouth of the River Amur.

The German expeditions will be provided with heliometers by Fraunhofer of 34 French lines (3 inches) aperture, and $3\frac{1}{2}$ feet focus; and the Russian expeditions with one heliometer similar to the German instruments, and two new heliometers, by

Repsold, of Hamburg, of greater power, viz., of 4 inches aperture, and $4\frac{1}{2}$ to 5 feet focus.

Messrs. Repsold have undertaken to complete for Lord Lindsay an instrument almost precisely the same as the large Russian heliometers, and as the instrument possesses some peculiarities a short description of it may not be uninteresting.

The new heliometer possesses the following advantages in common with the Oxford heliometer :—

1. The motions of the two halves of the objective take place in curved slides—slides which are portions of a cylinder whose axis passes through the focus of the object-glass at right angles to the optical axis of the telescope. Thus, whatever the separation of the two halves of the object-glass, the images remain constantly in the focus of the eye-piece. This was not the case in the Königsberg heliometer, and caused Bessel much inconvenience.

2. There is unlimited rotation of the whole tube with the micrometer handles in a cradle attached to the declination axis, and position angles are measured by this rotation on a circle attached to the telescope, and read from the eye end.

3. The micrometer slides of the divided object-glass read from the eye end.

Besides these advantages possessed in common with the Oxford instrument, the new heliometer has,—

1. An arrangement whereby both halves of the object-glass can be made to advance symmetrically and simultaneously in opposite directions.

This is of great advantage for obtaining rapid and symmetrical measures on both sides of zero, without altering the position of the object in the field of view. This mode of observation is always to be recommended for eliminating index error, and especially with the heliometer where the considerable weight of the two moving segments of the objective, and their continual change of position, render it peculiarly necessary.

2. The graduations of the two slides are arranged side by side, and so are both read off by the same microscope from the eye end.

3. There is an arrangement, movable from the eye end for the gradual shutting off either one or other half of the objective in order to equalise the brightness of the images.

4. There is a metallic thermometer at the objective end which can be read off from the eye end. We obtain from this thermometer the temperature of the scale,—a very essential matter, particularly where, as in our observations of the transit of *Venus* the temperature from the continual increase of the Sun's altitude will be rapidly rising.

It is but justice to the memory of the illustrious Struve to mention that most of these improvements in the heliometer were indicated, more or less clearly, in his remarks on the imperfections of the Pulkova heliometer in his description of that Observatory.

(*Description de l'Observatoire central de Pulkova, par F. G. W. Struve. Page 208.*)

Only the heliometrical part proper, of this instrument, viz., the cradle tube, and objective, will be made by Messrs. Repsold, who have undertaken to complete it by September, 1873. The equatoreal mounting is being made by Messrs. Cooke and is in a forward state.

The method of observation now suggested, derived solely from experience of measures made with Airy's Double Image Micrometer, we detail for the purpose of securing the advice and assistance of those who have already experience in the use of the heliometer, and so obtaining the best possible method of observation, which should be adopted by all observers of this transit with the heliometer.

We take it for granted that any method of observation should in itself, as far as possible, possess the means of eliminating the following sources of error:—

1. *Personality*, or habit of observation of the observers at different stations.
2. Effect of change of value of scale by temperature.
3. Effect of change of zero of scale.
4. Effect of periodic or accidental error of scale.
5. Effect of change of apparent diameter of Sun, or *Venus*, due to change of irradiation, from variable transparency of the atmosphere or other causes.
6. Entire neglect of position angles, for the measurements of which the method of double image is unsuited.
7. The method which otherwise fulfilling the above conditions, shall enable the observer to make the greatest number of observations.

The method we propose is best explained by reference to the accompanying figures.

The dotted lines indicate the image produced by one half of the objective which we shall call A. The black lines by that half which we shall call B. We shall call the images so produced Sun A, Planet A, and Sun B, Planet B.

Fig. 1. represents *Venus* about $8\frac{1}{2}$ minutes after first internal contact at the Mauritius, and the other figures, at intervals thereafter, each of about 5 minutes 18 seconds.

The dotted line indicates the line of displacement of the halves of the objective, which is of course the line of least distance of centres of the planet and Sun, at the instant of observation.

It will be seen that the measures follow each other without any break, and in direct order, except between 9 and 10 where the images are reversed in order to make the subsequent measures of the set occur on opposite sides of zero, and to obtain at the same time a measure of the Sun's diameter. After the completion of each set, the instrument is rotated 180° in order to give the greatest possible symmetry to the observations.

The arrangement is otherwise so natural and simple that it

appears to us at least to have the merit of being as quick as any that can be proposed.

Fig. 1 shows what we may call the external contact in line of centres of Planet B on Limb A, at least distance from limb.

Fig. 3. The internal contact of the same.

Fig. 11, 12, show the same opposite contacts, but at greatest distance from limb.

No further description will be required in explanation of the figures. Let us see how these measures combine to eliminate the errors referred to.

Nos. 2 and 5 combined give a measure of twice the diameter of *Venus*, and the index error, or zero point of the scale, as also do 15 and 16.

Nos. 9 and 10 combined give a double measure of the Sun's diameter, and another determination of the zero point.

From the values of the diameter of the Sun and *Venus*, and zero point of the scale thus found, we are enabled to treat each of the other measures as a complete measure in itself of the distance of centres, at the time of measurement in terms of the Sun's diameter.

Combinations of 1 with 3, of 4 with 6, and 13 with 14, eliminate any error dependent on the assumed diameter of *Venus*, in the measurement of the least distance of planet from limb of Sun.

Combinations of 7 with 8, and 11 with 12, eliminate errors dependent on the same cause in the measurement of the greatest distance of the planet from Sun's limb. And combinations of both these eliminate any error of the value of the Sun's diameter, and give the true distance of centres.

We have represented this series of observations as being taken at intervals of 5 minutes, 18 seconds, and occupying nearly half the time of transit.

We by no means think it likely that only so few measures can be taken, indeed we confidently expect to make at least five times as many, or to repeat the series ten times in the course of the transit.

Since in each series we have an independent determination of the Sun's diameter in terms of the scale, and, as we can safely assume for our purpose that that diameter is perfectly known for the time and place of observation (since the parallax sought is so small a fraction of that angle), if we could repeat the series sufficiently often so that the temperature might be considered uniform during the series, we should be independent of the effects of temperature on the micrometer scale.

As, however, clouds may interfere, or the temperature of the scale change very rapidly during observation, it is desirable to eliminate its (temperature) effect independently for each observation.

The method to be employed for this purpose enables us to investigate at the same time periodic or accidental errors of the scale, as follows :—

It is intended to mount the heliometer on one of the collimator piers of the 8 inches transit-circle of the Dunecht Observatory. This instrument, by Messrs. Simms, will be similar with some slight modifications to the new Cambridge circle.

It has two circles each of 3 feet in diameter, and each read by eight microscopes. The Alidade circles carrying the microscopes can be rotated and clamped at different points relative to the circles of the transit.

Thus when the heliometer is firmly mounted on one of the granite collimator piers, directed towards the transit, and the two semi-lenses separated vertically by a known number of scale divisions, a double image of a horizontal thread in the focus of the heliometer will be seen in the telescope of the transit-circle. The interval between the components of which will be the angular interval corresponding to the scale-reading at the temperature of observation.

This angle can be measured with the transit circle, free from errors of division of the circles, by the method of "iteration," *i.e.*, by changing the position of the microscopes relative to the divided circles, by rotation of the Alidade circles between each observation.

By artificially heating the transit-room with gas, the angular value of the same readings of the heliometer scale can be examined at different temperatures with comparative ease. Obviously also the different parts of the scale for uniformity.

If, however, the two scales are divided with the same engine, and in the same direction, it is obvious, that any periodic error of increase or diminution of value of the scale-intervals would be eliminated from the motions of the semi-lenses taking place in opposite directions; whilst, as it is impossible that during the transit the same scale readings can take place more than twice, (and that not very likely), the often-repeated observations at different parts of the scale would entirely eliminate accidental errors of division.

The observation of both limbs of the planet in the series eliminates any error that might arise from personality of the observer, either from a habit of making contacts too deep or too shallow, or, what would produce the same effect, from greater or less irradiation due to the instrument, the eye of the observer, or the state of the atmosphere. And similar errors which might arise from change of irradiation are eliminated from having opposite effects on measures of greatest and least distance of the planet from the limb.

Since the Sun as a whole is sensibly circular, and the measures are distributed over two opposite arcs of its circumference each of about 60° , any irregularities of its periphery temporarily or permanently existing will thus be eliminated.

We cannot help thinking, it is here the heliometric and photographic methods possess so strong a claim to preference over those of mere contact, where any temporary outburst of solar

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We cannot help thinking, it is here the heliometric and photographic methods possess so strong a claim to preference over those of mere contact, where any temporary outburst of solar

energy may occasion a bulging out of the point of contact, or the presence of very bright faculæ causes abnormal irradiation.

With regard to the degree of accuracy of which the heliometric method is capable, it is not easy without trial to arrive at definite conclusions, but we think that on an object like the Sun, and with an instrument such as that described, constructed specially for its observation, it is not extravagant to suppose that the probable error of a single observation should not exceed half a second of arc.

Now each series contains ten measures, the probable error of one complete series should therefore be $\frac{0.5}{\sqrt{10}} = 0''.160$. If only ten such series were obtained, the probable error of least distances of centres should be about $0''.050$.

Now at Lake Baikal the factor of parallax in distance of least centres is about 0.9 , and the Mauritius about 0.3 .

If we take the horizontal parallax of *Venus* referred to the Sun, to be $23''.5$ the total difference of least centres will be

$$23''.5 \times (0.9 + 0.3) = 28''.2$$

Taking the Sun's horizontal parallax at $8''.9$ the effect of this probable error of measurement of least distance, viz., $0''.056$ on the deduced solar parallax would be

$$0''.050 \times \frac{8''.9}{28.2} = 0''.016$$

or the probable error of the result considerably less than two hundredths of a second.*

We are in hopes that the probable error of a single observation will be found to be less than half a second, and that more than ten complete sets of observations will be obtained during the transit.

If even we are disappointed in this we think the heliometric method has still been shown to be capable of giving very highly accurate results.

There are many matters of detail which we have fully or partially considered which cannot find a place here, but we trust the Society will receive charitably what only pretends to be a sketch of the direction in which we are working, and an appeal for assistance in advice and co-operation.

Duneeht, Oct. 1872.

Since the above was written we have received the following letter from Prof. Auwers of Berlin, which is so full of interest, that with his permission we insert it. (Translation.)

In reply to your letter of the 28th of October, and received

* This is only strictly true of measures at times of greatest phase, but sufficiently near an approximation for our present data.

yesterday, I can add very little to what I wrote on Jan. 28th. Though I promised you then further communications as soon as the intentions were advanced beyond mere projects, I am not in a position now to fulfil my promise. I was in hopes then that last spring the proposals of the committee would have had the consent of the administration, which is, however, not yet the case. We are, however, empowered by government to order all the necessary instruments, which means that our proposals will be accepted in their entirety.

Three chief expeditions will be sent: one probably to the harbour of Chefoo, in China; one to the Auckland Islands; and one to the Macdonald Islands; but in the event of these islands presenting too many difficulties, the last-named expedition would be despatched to the Kerguelen Islands.

These three expeditions would direct their attention to—

(1.) Heliometric measurements of the distance of *Venus* from the nearest and farthest point of the circumference of the Sun during the whole time of the transit.

(2.) Observation of time of first and last contact.

(3.) Photographic pictures during the transit from which the distance as well as the angle of position of *Venus* in relation to the centre of the Sun may be measured.

(4.) Another expedition will be sent to the Mauritius, entrusted with the observations 1 and 2, and

(5.) A photographic expedition only to Persia.

The instruments to be used are:—

(1.) Four heliometers made by Fraunhofer (34 Paris lines apertures and $3\frac{1}{2}$ feet focus) with alterations and improvements.

(2.) Four equatoreals by Fraunhofer, 52 lines aperture and 6 feet focus.

(3.) Two photographic telescopes by Steinheil, with achromatic object-glass $5\frac{1}{2}$ inches aperture, and 2 photographic apparatus by Steinheil, with quadruple object-glass of 4-inch aperture.

In addition, every station will have the necessary instruments for observations of time and place, and one or more smaller telescopes for the observations of first and last contact.

To the stations in the Southern Hemisphere (where the longitude must be found by observations of the Moon), transit instruments will be sent with diagonal telescopes of 30 lines aperture and altazimuths with 12 to 14-inch circles. At the station of the Mauritius, it is the intention to observe with these instruments the culmination and altitude of the Moon, and with the refractor as many star-occultations as possible.

I cannot state to-day the names and situations of the Russian stations, as I have no access to the protocols of the Russian commission at present. A great number of stations between the Caspian Sea and the mouth of the Amour will be established chiefly for observations of first and last contact. The chief expeditions go to the east of Siberia, each with an heliometer of

4 inches, one telescope of 6 inch aperture, and a photographic instrument. These latter are being made by Dallmeyer in London, nearly after the pattern of the Kew-heliograph.

I shall be much interested to hear about Lord Lindsay's intentions through the promised paper at the Royal Astronomical Society.

On the Origin of the November Meteors.

By Richard A. Proctor, B.A. (Cambridge.)

Although the researches of Schiaparelli, Adams, Leverrier, and others, have demonstrated the nature of the orbit of the November meteors, and the existence of an association of some sort between these bodies and Tempel's comet (Comet I, 1866), yet the manner in which these and other meteors have entered our system remains as yet unexplained. Schiaparelli's theory on this point does not stand by any means in the same position as his theory respecting the association between meteor-systems and comets. Indeed it is impossible to consider carefully all the circumstances known respecting meteors on the one hand and respecting the interstellar regions on the other, without recognising very grave difficulties in the views of Schiaparelli.*

I wish to invite attention here, however, to those difficulties only which surround the theory that the November meteors entered the solar system from the interstellar spaces, and were forced to take up their present orbit by the attraction of the planet *Uranus*. It is known that this theory has been adopted, or at least supported, by no less an authority than Leverrier; and this might appear at a first view to render further inquiry superfluous, since no one will suppose that any considerations bearing on the dynamical relations of the question could be overlooked by Leverrier. But as a matter of fact the support of Leverrier has been accorded only to the general theory that a body arriving from interstellar space might be forced by the attraction of *Uranus* to take up an orbit like that of the November meteors. Leverrier has also shown that without any extravagant suppositions as to errors in the observations of the November meteors and Tempel's Comet, the comet might, in the year 126 A.D., have been near enough to *Uranus* to have its orbit changed to its present form. Against these propositions I have nothing to urge; but I wish to invite attention to the inquiry whether the event admitted to be possible in this case be not so highly improbable as to suggest that some other explanation of the circumstances should be looked for.

Considering the case of a single body—as a single particle—

* The association which he traces between nebulae and comets must, in particular, be regarded as open to strong objections, whether we consider the spectroscopic evidence or the evidence which we have respecting the enormous distances of all the known nebulae.

arriving from interstellar space (on a path inclined to the fixed plane of the solar system), and eventually, owing to the attraction of *Uranus*, forced to follow the path of Tempel's Comet, we have the following change in the body's circumstances to account for:—

Whereas arriving from outer space under the Sun's attraction such a body would cross the orbit of *Uranus* with a velocity of about 6 miles per second, in its new path it has at the same distance a velocity of about $1\frac{1}{2}$ miles per second. The action of *Uranus* must have been so exerted as to deprive the body of a velocity of about $4\frac{1}{2}$ miles per second.

Now *Uranus* acting alone on a body in the interplanetary spaces, and drawing that body to his surface, would impart to the body a velocity of about 13·7 miles per second. Such a body when at the distance of *Uranus*'s nearest satellite (about 86,000 miles) would have acquired a velocity of about $4\frac{1}{2}$ miles per second. And since the velocity which a body can impart corresponds to the velocity which it can take away, it follows that *Uranus* could deprive a body of a velocity of $4\frac{1}{2}$ miles per second, supposing the body to start with such a velocity from the distance of the nearest satellite.

But it need hardly be said that under the actual circumstances in which the solar system exists, *Uranus* could not even impart or take away such a velocity as this from a body starting from the distance of the nearest satellite or approaching *Uranus* within that distance. In particular it is manifest that a body arriving from outer space under the Sun's attraction, and passing *Uranus* at the distance of the nearest satellite, would by its own motion, augmented by the effects of the Sun's attraction, be carried away from the influence of *Uranus* at such a rate that that influence, though tending to reduce the body's velocity (that is, under the actual circumstances preventing the velocity from increasing so fast as it otherwise would), would nevertheless not produce a total reduction of $4\frac{1}{2}$ miles per second. For such a change to be produced the body must pass much nearer to *Uranus*.

A fortiori, therefore, it follows that a body passing *Uranus* with a velocity of 6 miles per second must approach the planet much more nearly than its nearest satellite. And the inference is yet further strengthened by the consideration that as the body approached *Uranus* the planet's attraction would tend to increase the velocity of the body; it would only be the excess of the retarding action (owing to the relative motions of *Uranus* and the body) which would avail to reduce the body's motion on the whole.

The conclusion is, that such a body must pass very close indeed to *Uranus* to receive the requisite degree of retardation. We need not enter into numerical calculations, because in our ignorance of the actual circumstances under which according to the theory, the members of the meteor system approached *Uranus*, calculation would be more laborious than profitable. But it can

readily be shown that to produce the retardation required, *Uranus* must have been within 30,000 miles of a body arriving from interstellar space, and eventually travelling in such an orbit as that of the November meteors.

Even, however, if we only assume that a body must have passed within 80,000 miles of *Uranus* to receive the requisite retardation, we at once recognise several serious difficulties.

In the first place, the antecedent improbability of so close an approach, in the case of a body arriving from interstellar space, after a journey lasting several millions of years, is so enormous that we might on that account alone object to the theory that such an approach has in reality taken place. To give anything like a reasonable chance of such an approach in any single case the total number of meteor systems thus arriving from without should exceed incalculably even the enormous number of such systems which we begin to recognise as actually existing within the solar scheme.

But this is not all. We know that at present the November meteor system,—counting only those members which travel nearly enough to the orbit of Tempel's Comet to come under the reasoning here employed,—has an extension measured by millions of miles in breadth and depth, and by hundreds of millions of miles in length. It seems impossible to explain how all these bodies can once have been gathered within so small a space that the whole system was set travelling on its present orbit by the disturbing influence of *Uranus*. If ever so compact, the system should have been able by the mutual attraction of its members to maintain its compactness for a very long time—at least not be scattered over a space hundreds of millions of miles long, within the astronomically short interval of seventeen centuries.

One is thus led to doubt whether the November meteors have had an extra-planetary origin at all. They must once have been, all in a compact body, so near to *Uranus*, that the idea is suggested that they came from *Uranus*; that in fact he expelled them in some tremendous volcanic outburst. Strange as this may sound, it is after all not a whit more strange than the theory (which seems forced upon us by other evidence) that meteoric bodies have in some instances been expelled from our Sun or from his fellow-suns the stars. If we suppose that at any former epoch *Uranus* was in a sunlike state, it would have required no greater proportional expenditure of power in that small sun to expel meteoric matter than for the central Sun to overcome by his explosive energy the might of his own gravity. *Uranus* would have had to impart but a velocity of 13·7 miles per second to expel matter from him for ever—even if we leave out of account the way in which the Sun's action would help in bearing away matter once carried to a certain distance from *Uranus*.

Of course, similar considerations can be extended to all the planets, minor as well as major—nay, they could be extended even to the secondary planets.

It would be a natural consequence of such explosive actions as have here been suggested, that each of the greater planets would have a dependent family of meteor systems, or comets, revolving in orbits approaching very near to the orbits of their respective parent planets. We might not, or rather we certainly should not, be able to detect from the Earth one in many hundreds of these dependent comets; and if any were detected, the just inference would be that an enormous number of such comets existed. I need hardly remind those who will read these lines that many comets depending in just such a manner on the planet *Jupiter* have been already detected; that the case is the same with *Neptune*; that there is one comet at least, which on this view of the matter must be regarded as Saturnian; and that of course the system of bodies I am considering would be regarded as a dependant of the planet *Uranus*.

A rather singular result would follow (as Professor Herschel has reminded me) from the theory here considered. Comets expelled from *Jupiter*, partaking of his rapid motion of advance would be found to travel for the most part in the same direction as the planet. Comets expelled from the much more slowly moving *Neptune* would be as likely to travel in either direction. This agrees with observation. All the comets whose aphelia lie near the orbit of *Jupiter* advance; a considerable number of those whose aphelia lie near the orbit of *Neptune* regrede.

Lastly, it is obvious that, according to this theory, one or other of the nodes of each of these comets should lie very close to the orbit of the planet from which the comet had been expelled. Now the nodes of all the Jovian and Neptunian comets, as well as of the Saturnian comet and Tempel's (the Uranian) comet, lie near to the orbits of their ruling planets. In the case of Encke's comet, whose aphelion is now far away from *Jupiter's* orbit, and gradually drawing inwards away from it, the further node is necessarily very far from the orbit of *Jupiter*. But this node lies as close to that orbit as is possible under the circumstances, being so near to the aphelion that one may say the line of nodes coincides with the line of apses.

On Two probable Early Appearances of the Comet of the November Meteors (1866, I. Tempel.) By Mr. Hind.

Some ten years since I calculated three or four orbits for the comet observed by the Chinese during the last week in October, 1366, somewhat varying in each case the interpretation of the path described in their annals, as it is presented by M. Edouard Biot in the appendix to the *Connaissance des Temps* for 1846. The orbits bore a sufficient general resemblance to indicate the possibility of arriving at a correct idea of the elements, though on one point in the interpretation there remained a doubt.

When the similarity of the orbit of the November meteors with that of the first Comet of 1866, discovered by M. Tempel, was pointed out by Dr. Peters, I remarked that it also presented considerable resemblance to the orbits I had deduced for the Comet of 1366, and the probability that this was an early appearance of Tempel's comet immediately occurred to me. Under these circumstances I applied to our Assistant-Secretary, Mr. Williams (whose extensive acquaintance with the Chinese language and Chinese astronomy is well known to the Fellows of this Society) requesting his aid in clearing up the doubt I have alluded to; and I am indebted to Mr. Williams for enabling me to state, with what I conceive a high degree of probability, that the Comet of 1366 (which does not appear to have been remarked in Europe) was the one now known to be associated with the November meteor stream.

Now, in October 1366, we have recorded a most imposing shower of meteors, one of the November series, discovered by Professor Newton. It was observed on the banks of the Tagus, and also in Bohemia. Humboldt gives us a description from a Portuguese Chronicle, which is thus translated in one of our editions of *Cosmos*: "In the year 1366, and xxii. days of the month of October being past, three months before the death of the King Dom Pedro, there was in the heaven a movement of stars, such as men never before saw or heard of. At midnight, and for some time after, all the stars moved from the east to the west; and after being collected together, they began to move, some in one direction and others in another. And afterwards they fell from the sky in such numbers, and so thickly together, that as they descended low in the air, they seemed large and fiery, and the sky and the air seemed to be in flames, and even the Earth appeared as if ready to take fire. That portion of the sky where there were no stars, seemed to be divided into many parts, and this lasted for a long time." Then follows a reference to the "great fear and dismay" which this phenomenon occasioned.

We have here, I do not doubt, a description of the appearances in the heavens occasioned by the proximity of a comet, for it is certain that the comet observed in China was close upon the Earth at the time of this memorable display, and as I have stated it appears highly probable that this was the comet in the track of which these swarms of meteoric bodies revolve.

It is not unlikely that in the rich store of cometary records which Mr. Williams has lately enabled us to consult, several other appearances of the meteor-comet of November may be recognised. At present I have only succeeded in finding one, in addition to that of 1366, and in this case it is the European Chronicles which put us in possession of the track of the comet. In 868 at the end of January a comet was observed under the tail of *Ursa Minor*, which moved in seventeen days almost to the constellation *Triangulum*. In China it was seen in the 1st Moon (February) with the same right ascension as stars in *Aries*

and *Musca*. I find, by calculation, that when Tempel's Comet arrives at perihelion at the end of March or early in April, it must follow this path in the heavens, being first situated at the end of January in the constellation *Camelopardus*, when, for want of conspicuous stars of reference, it might be said to be below the tail of *Ursa Minor*, afterwards moving to *Triangulum* and *Aries*.

Between 1866 and 1366 we should have fifteen periods of 33[·]28 years, and between 1366 and 868, also fifteen periods of 33[·]24 years.

Note on the First Comet of 1818 (Pons, February 23).

By Mr. Hind.

In the last number of the *Monthly Notices*, Professor Herschel, following Dr. Weiss, alludes to a possible connexion of the first Comet of 1818, at some previous time, with Biela's Comet. The hypothesis is founded upon an apparent similarity of an orbit calculated by Mr. Pogson for the Comet of 1818 to that of Biela, but it is one that will not bear examination.

The particulars of Pons' observations, or rather estimation of the positions of the Comet of 1818, will be found in *Zeitschrift für Astronomie*, vol. v. p. 148. Four places are given with two obvious errors. Correcting these, I have endeavoured to find the orbit which would best accord with the rough data, and have fixed upon the following elements:—

Perihelion Passage, 1818, February 3[·]218 G.M.T.

Longitude of Perihelion	76° 18'
Ascending Node	256 1
Inclination	34 11
Distance in Perihelion	0.6959

Motion direct.

These elements cannot be said to favour the supposed connexion of the Comet with that of Biela.

But the direct calculation of the orbit from such imperfect data for comparison with that of the periodical comet, is not in this case the most legitimate or satisfactory method of putting the above supposition to the test. We know pretty nearly what were the elements of Biela's Comet in 1772 when it was first observed, and we have accurate determinations of the orbit in 1806 and 1826. We can therefore ascertain whether it is possible to represent the observed position of Pons' comet at discovery by the elements of Biela's. I first adopt the elements of 1826, and find that with a true anomaly = $-37^{\circ} 55'$, the observed and computed longitudes would agree, but the difference of latitudes is no less than 26° . Again, if the orbit for 1772 is employed, the inclina-

tion in that year being nearly 4° greater than in 1826, I find with true anomaly $= -47^{\circ} 36'$, we have an agreement in longitude, while the difference of latitudes is increased to 29° .

These large differences appear conclusive against the idea of a possible connexion of the first Comet of 1818 with the Comet of Biela.

On the 'Rate of a Clock going in a Partial Vacuum.

By R. C. Carrington, F.R.S.

It is not easy to get a vacuum at all, I mean of the least degree, for one is soon reminded that Nature abhors a Vacuum. First, I had to abandon a mahogany case made for me by the late Mr. Frodsham as utterly useless, and to order another one made of copper, which after being tried and tried again, and repainted inside and out, at last proved tight. Then came the glasses, which necessarily enclose the front, to enable the height of the barometer within and the temperature within to be read. Suffice it to say, that I have broken eight pieces of glass, half an inch thick each, at an expense of £3 for each, and that now I have a plate with faults in it. Firstly I observed γ *Draconis* on three successive nights, the 29th, 30th, and 31st of August, 1872, and found the following errors,—

	^s
Aug. 29th	3'25 fast on N. Alm.
30	4'37 „
31	6'71 „

the barometer standing at $27^{\text{in}}.50$ from the 29th to the 30th, and at $26^{\text{in}}.00$ from the 30th to the 31st. These give accordingly,—

gaining $1^{\text{m}}.12$	and $2^{\text{m}}.34$
at $27^{\text{in}}.50$	„ $26^{\text{in}}.00$

$$\text{or } \frac{1^{\text{m}}.22}{1^{\text{in}}.50} = 0.8133 \text{ for 1 inch midway between } 27^{\text{in}}.50 \text{ and } 26^{\text{in}}.00.$$

The observations were very good; in fact, could hardly be better. The telescope was not moved during the three days.

On September 16, I commenced another set at $28^{\text{in}}.00$ barometer, and continued them on the 17th and 19th; then at $29^{\text{in}}.00$ on the 19th, 20th, and 21st; then at $28^{\text{in}}.50$ for the 21st, 22nd, and 25th; then at $28^{\text{in}}.00$ for the 25th, 26th, and 28th; then at $27^{\text{in}}.50$ for the 28th and 29th and October 3rd; then at $27^{\text{in}}.00$ for October 3rd and 6th, when the glass broke again and stopped me. The observations were mostly those of ζ , δ , γ , α , and β *Aquilæ*, and were not so accordant as could be desired. Nevertheless, I give the following as the resulting rates per diem,—

for 29°00	gaining rate — 0 ^s .62 per diem	temp. 52 ^o .9 F.
28°50	„ — 0°95 „	„ 52°8 F.
28°00	„ — 1°38 „	„ 52°3 F.
27°50	„ — 1°72 „	„ 51°9 F.
27°00	„ — 2°01 „	„ 51°5 F.

Combined with the former result by γ *Draconis*, I infer the following as the coefficient for barometer,—

$$+ 0^{\circ}.720 (b - 27^{\circ}.5)$$

and I assume — 1°70 for 27°5. There results,—

for 29°0	— 0°62	Error °00
28°5	— 0°98	+ °03
28°0	— 1°34	— °04
27°5	— 1°70	— °02
27°0	— 2°06	+ °05
26°5	— 2°42	—
26°0	— 2°78	— °14

I have to point out that in both series, the observations were taken with falling barometer, and to add that I wish to take them again from 26°00 to 29°00 rising. But noticing that the pendulum was becoming rusty, I discontinued observations after the last break, and took the pendulum down, and have had it electrogilt. I have remounted it, and rated it approximately, but now I am without the object-glass to the altazimuth. When that is returned, I shall resume the subject.

The resulting coefficient 0^s.720 may be of value, as I see by Sir G. B. Airy's paper on the "Figure of the Earth," in referring to the observations made by M. Carlini on Mont Cenis compared with those of M. Biot at Bordeaux. See *Encyc. Met.* "Fig. of the Earth," p. *239.

I have only to add that this paper affords the best answer, a complete negative, to one formerly written by a horologist, to show that for a pendulum swinging 2° on either side of 0, the barometer correction was nothing.

On the Parallax and Proper Motion of Lalande 21185.

By W. T. Lynn, Esq.

In vol. xlviii. p. 291, of the *Astronomische Nachrichten*, there is a paper by Dr. Winnecke, containing a provisional determination of the annual parallax of this star, which is of the seventh magnitude, and was discovered in May, 1857, by Prof. Argelander

to have a very large proper motion.* Dr. Winnecke calls it Argelander's second star, and has recently published (as No. xi. of the publications of the German Astronomical Society) his definitive determination of its parallax, with the details of the calculation. The result is an annual parallax of $0''.501$, with probable error $\pm 0''.01138$, corresponding to a distance of 412000 in terms of the Sun's mean distance from the Earth, over which it would take light $6\frac{1}{2}$ years to traverse.

It may be worth while to determine the proper motion of this star from recent meridional observations at Greenwich. It was observed in 1859 and in 1861, which observations are included in the two Seven-year Catalogues, for 1860 and 1864 respectively. In their reduction, account was not taken of the proper motion. It is desirable, therefore, to give here the corrected places, taking into account the approximate proper motion ($-0''.05$ in R.A., and $+4''.7$ in N.P.D.); and then, by comparing these with an observation made at Greenwich in April of the present year, we have the means of deducing a tolerably accurate value of the proper motion.

Two observations were made in 1859, the results of which, corrected for the above approximate proper motion, give for mean place on 1859, Jan. 1,

R.A. $10^h 55^m 37''.26$ N.P.D. $53^\circ 5' 11''.09$.

This would give for the reduced mean place in 1860, Jan. 1,

R.A. $10^h 55^m 40''.56$ N.P.D. $53^\circ 5' 34''.86$

instead of the numbers given in the Seven-year Catalogue for 1860.

An observation of N.P.D. only by reflection was made in 1861. The mean of the two results by direct and reflected vision gives, similarly corrected, for 1861, Jan. 1,

Mean N.P.D. $53^\circ 5' 57''.30$.

From this the resulting Mean N.P.D. on 1864, Jan. 1, will be

$53^\circ 7' 9''.24$,

instead of the value given in the Seven-year Catalogue for 1864.

* A brief notice of Dr. Winnecke's preliminary determination of the parallax of this star is inserted in vol. xviii. of the *Monthly Notices*, p. 289. The annual parallax obtained from this first discussion of the observations is $0''.511$, which scarcely differs from the resulting value of Dr. Winnecke's recent and more elaborate investigation. Prof. Argelander's value of the proper motion of *Lalande 21185*, determined in 1857, is $-0''.047$ in R.A., and $+4''.73$ in N.P.D.

The result of the observation made on 1872, April 10, is

Apparent R.A. $10^h 56^m 21^s.33$ Apparent N.P.D. $53^\circ 10' 20''.18$,

or, reduced to Mean R.A. and N.P.D. 1872, Jan. 1, allowing for the fractional part of the proper motion,

Mean R.A. $10^h 56^m 20^s.22$ Mean N.P.D. $53^\circ 10' 22''.15$.

By comparing the result of the observations in 1859 with that of the observation in 1872, we obtain for the proper motion,

In R.A. $- 0^s.044$

In N.P.D. $+ 4''.66$

which are almost identical with the values determined by Prof. Argelander in 1857.

This is the third largest stellar proper motion which has been detected, ranking next after *Groombridge* 1830 and 61 *Cygni*. *Lalande* 21185 appears at least to equal the latter in parallax.

Ephemeris of the Angle of Position and Distance of the Binary Star α Centauri, about the approaching Periastron Passage.
By Mr. Hind.

The following ephemeris exhibiting the interesting changes which are about to be presented by this binary, is calculated from the elements communicated to this Society by Mr. Eyre B. Powell, of Madras, which are founded upon the measures up to 1870 inclusive, and are the best hitherto published. The nearest approach of the components takes place about 1875.29, when the distance is $1''.2$. It is to be hoped the southern observers will give close attention to this object during the next few years, as more than an average lifetime must elapse before the opportunity again occurs, for procuring most essential data for the accurate determination of the orbit.

1872.00	25.05	8.634	1875.25	98.23	1.209
72.50	26.71	7.859	75.50	137.17	1.292
73.00	28.74	6.900	75.75	162.15	1.796
73.50	31.56	5.744	76.00	175.06	2.485
74.00	36.01	4.402	77.00	191.67	5.414
74.50	44.59	2.967	78.00	196.73	8.138
75.00	68.58	1.614			

If good measures are secured in 1873, we may be able to make a closer prediction of the rapid changes indicated by Mr. Powell's elements, in the latter half of 1874 and in 1875.

Mean Places for 1871, January 1, of 78 Stars near the South Pole, observed at the Royal Observatory, Cape of Good Hope, in the Year 1871.

(Contributed by E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer at the Cape of Good Hope.)

No.	Stars' Names.	Mag.	Number of Obs. above and below Pole.	Total Number of Obs.	Mean R.A.			Frac- tion of Year.	Mean N.P.D.			Brisbane.	B.A.C.	Johnson.
					h	m	s		1871.	°	'			
1	Lacaille 9745	7	{ S.P. 4 } 2	6	0	1	54.28	.49	176	45	26.83			
2	" 23	6	{ S.P. 1 } 1	2	0	8	22.79	.51	175	42	46.02	18		40
3	Octantis	6.5	3	3	0	13	5.35	.58	179	4	49.15	32		71
4	Lacaille 228	7	{ S.P. 3 } 1	4	0	36	25.29	.54	175	57	37.54			
5	" 242	7	{ S.P. 1 } 1	2	0	38	50.69	.47	176	7	11.72			
6	" 248	6.5	{ S.P. 3 } 1	4	0	39	53.75	.54	176	24	29.50			
7	" 293	7	{ S.P. 4 } 2	6	0	46	8.91	.53	176	35	46.07			
8	" 634	6	{ S.P. 6 } 6	12	1	45	7.74	.54	175	25	12.34			
9	" 760	7	{ S.P. 2 } 3	5	2	3	46.61	.52	175	39	38.01			
10	" 764	6	{ S.P. 3 } 3	6	2	5	48.12	.53	175	22	21.98			
11	" 1029	7	{ S.P. 3 } 10	13	2	40	14.61	.52	176	17	10.55			
12	" 1146	7	{ S.P. 1 } 4	5	2	51	58.96	.50	175	33	35.44			
13	" 1884	7	{ S.P. 2 } 6	8	2	53	32.08	.55	178	57	4.10			
14	" 1203	7	{ S.P. 2 } 9	11	3	4	35.28	.53	176	22	54.69			
15	" 1848	7	{ S.P. 2 } 7	9	3	22	32.13	.57	178	40	47.62			
16	" 1414	7	S.P. 9	9	3	47	20.23	.53	175	8	13.86			
17	" 1592	6.5	S.P. 6	6	4	6	46.03	.56	175	38	14.06			
18	" 1839	7	S.P. 7	7	4	37	30.85	.50	176	33	1.91			
19	" 2296	6	S.P. 6	6	5	55	13.87	.46	174	50	28.24	1165		1960
20	" 2512	6	S.P. 2	2	6	13	45.33	.57	175	55	28.17	1269		2085
21	" 3274	6.5	S.P. 6	6	7	31	24.85	.51	176	48	37.42			
22	Brisbane 2007	8	S.P. 4	4	8	8	45.55	.50	175	34	13.96			
23	A Octantis	7	S.P. 4	4	8	13	14.00	.57	178	29	27.53	2298		2878
24	Lacaille 3759	6.5	S.P. 4	4	8	45	19.44	.48	176	7	6.80	{ 2302 } { 2308 }		
25	Octantis, Lac. 3953	5.5	S.P. 5	5	9	14	55.90	.48	175	8	34.65	2491		3211

No.	Stars' Names.	Mag.	Number of Obs. above and below Pole.	Total Number of Obs.	Mean R.A. Jan. 1, 1871.			Frac- tion of Year. 1871.	Mean N.P.D. Jan. 1, 1871.			No. in Brisbane. B.A.C. Johnson.
					h	m	s		°	'	"	
26	*	{ 7.5 7.0 }	S.P. 3	3	9	48	25.76	.53	175	25	7.18	
27	Lacaille 4169	6.5	S.P. 7	7	9	48	39.56	.54	175	25	6.72	
28	" 4342	7	{ S.P. 1 4 }	5	10	11	53.56	.48	176	16	56.40	
29	*	7	{ S.P. 2 3 }	5	10	31	10.14	.45	175	53	56.62	
30	*	7	S.P. 3	3	10	31	13.83	.57	178	51	32.34	
31	Lacaille 4510	6.5	{ S.P. 3 6 }	9	10	38	19.48	.45	175	25	16.95	
32	" 4578	7	{ S.P. 2 7 }	9	10	47	35.38	.46	176	13	11.83	
33	" 4708	7	S.P. 3	3	11	7	50.11	.53	175	2	59.30	3491
34	" 4731	7	S.P. 3	3	11	10	49.00	.54	175	31	46.72	3525
35	Brisbane 3618	7	{ S.P. 3 4 }	7	11	23	28.19	.47	174	14	42.69	3618
36	*	7	{ S.P. 1 3 }	4	11	26	23.86	.53	178	32	1.86	
37	Lacaille 4865	7	{ S.P. 3 4 }	7	11	34	27.70	.48	174	46	21.04	3722
38	" 4991	6	{ S.P. 3 4 }	7	11	55	57.79	.47	174	54	48.16	3884 4058
39	Brisbane 3962	7	{ S.P. 1 2 }	3	12	7	27.35	.51	177	41	52.67	3962
40	Lacaille 5107	6	{ S.P. 4 2 }	6	12	15	32.81	.47	175	26	5.39	4018 4164
41	Brisbane 4091	7	{ S.P. 2 3 }	5	12	32	25.19	.52	179	5	26.49	4091
42	Octantis Lac. 5268	5	{ S.P. 2 2 }	4	12	41	41.67	.50	174	25	17.69	4187 4293
43	Lacaille 5325	7	{ S.P. 5 4 }	9	12	53	9.70	.49	176	51	53.02	4253
44	" 5452	7	{ S.P. 3 3 }	6	13	15	39.93	.50	175	9	17.85	4410 4460
45	" 5444	7	{ S.P. 4 3 }	7	13	16	42.42	.51	176	3	32.99	
46	Octantis Lac. 5482	5	{ S.P. 5 4 }	9	13	20	32.50	.51	175	7	20.07	4445 4483
47	*	{ S.P. 1 5 }		6	13	28	39.49	.54	176	58	13.75	
48	Brisbane 4614	{ S.P. 7 4 }		11	13	55	44.07	.53	178	46	55.69	4614
49	Octantis Lac. 5823	6.7	{ S.P. 7 2 }	9	14	27	42.60	.53	177	36	52.75	4886 4790 327
50	Lacaille 5882	7	{ S.P. 4 1 }	5	14	29	24.14	.50	175	56	8.08	
51	" 6441	7	4	4	16	8	55.21	.52	177	19	13.52	

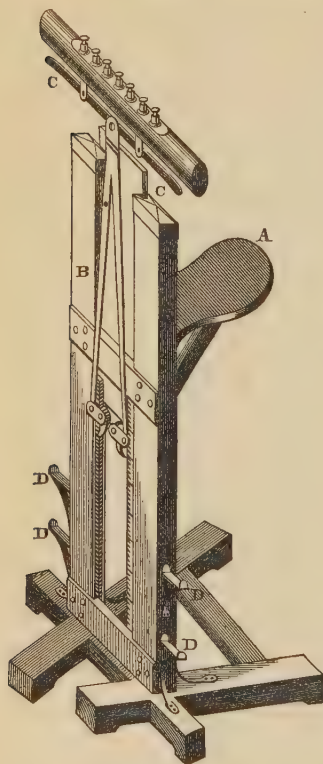
											No. in			
No.	Stars' Names.	Mag.	Number of Obs. above and below Pole.	Total Number of Obs.	Mean R.A.			Frac- tion of Year.	Mean N.P.D.			Brisbane.	B.A.C.	Johnson,
					Jan. 1, 1871.				Jan. 1, 1871.					
					h	m	s	1871.	°	'	"			
2	Brisbane 5607	6.7	5	5	16	13	25.33	.50	176	6	34.06	5607	5412	388
3	*	7	4	4	16	51	26.51	.56	177	7	32.39			
4	*	7	3	3	16	51	29.35	.56	177	15	12.51			
5	Lacaille 7078	7	6	6	17	20	58.38	.56	175	9	2.38			
6	Brisbane 6058	6	8	8	17	38	51.49	.49	177	39	15.44	6058	5936	433
7	" 6229	7	5	5	17	58	56.40	.55	176	16	6.77	6229		
8	Octantis Lac. 6295	6	7	7	18	8	11.93	.50	179	16	42.00	5912	5959	423
9	Lacaille 7442	7	2	2	18	13	59.24	.56	175	40	37.16			
10	" 7751	7	8	8	18	51	55.28	.49	174	56	5.50			
11	" 8257	7	2	2	20	11	24.13	.51	174	50	12.41			
12	*	7	4	4	20	45	55.48	.56	175	42	48.03			
13	*	6	3	3	20	48	54.78	.48	176	9	38.31			
14	Lacaille 8474	7	3	3	20	49	50.76	.53	175	16	20.76			
15	" 8511	7	3	3	20	54	2.37	.51	174	50	2.33			
16	B Octantis	6.7	6	6	20	57	19.03	.51	179	26	24.17	6644	7020	496
17	Lacaille 8551	7	4	4	21	3	40.69	.49	175	21	19.08			
18	" 8626	7	4	4	21	20	42.74	.50	176	25	9.16			
19	" 8751	7	3	3	21	27	34.58	.48	174	32	52.76			
20	" 8720	6.7	6	6	21	27	46.32	.51	175	37	29.53			
21	" 8738	7	5	5	21	40	16.33	.49	177	5	49.44			
22	C Oct. Lac. 8924	6	{ S.P. 7 1 }	8	22	6	8.90	.51	176	37	9.91	7119	7713	549
23	Lacaille 9123	7	{ S.P. 3 1 }	4	22	29	13.97	.47	174	24	50.16			
24	Octantis		{ S.P. 6 3 }	9	23	7	26.93	.46	178	11	20.58	7421		578
25	Lacaille 9401	7	{ S.P. 5 3 }	8	23	15	51.82	.44	176	25	4.80			
26	" 9464	7	{ S.P. 3 1 }	4	23	26	32.36	.51	177	6	40.72			
27	" 9563	7	{ S.P. 4 1 }	5	23	38	55.25	.49	174	34	45.08			
28	" 9596	7	{ S.P. 4 3 }	7	23	43	59.15	.49	176	36	49.27			

1872, October 3.

Note on an Observing Chair for use with Reflecting Telescopes of the Newtonian Construction. By John Browning.

The model of the observing chair, which I now exhibit, is made principally from suggestions I have received from Mr.

Knobel. I have, however, at his kindly expressed wish, introduced several modifications which he thinks will add to its usefulness, before bringing the contrivance forward for practical use.



The seat A, on which the observer sits astride, is rather more than counterbalanced by two weights attached to cords which hang from pulleys at the top of the uprights, and the weights run down the uprights B B, which are made hollow for the purpose; the chair is always locked automatically, but can be released by the observer clasping either of the small metal handles C C underneath the main bar at the top which moves with the seat. On releasing the cams by means of either of these handles, and simply raising himself from the seat, by resting his feet on the projecting brackets D, D, D, D, the seat will itself rise in position, while by allowing his weight to rest gently on the seat while either of the cam levers are held, he can lower the seat to the full extent of the actions.

On a Modified Form of Solar Eye-piece.

By John Browning.

Having been recently engaged in constructing several solar eye-pieces, the object in each case being to produce an eye-piece which would reflect to the eye of the observer fewer rays than those in general use, I came to the conclusion that the ordinary form of solar eye-piece, that is, with a single-surface reflexion from a prism or prisms, has not hitherto been made in the most efficient form.

It must upon the slightest consideration be at once evident that instead of presenting the surface of each prism to the light at an angle of 45° , so that the light should be reflected at an angle of 90° from the surface, that much more light would be transmitted, and much less reflected if the prism were presented to the light at an angle of nearly 90° , or as near as the difficulties of practical construction would admit of.

I have made an eye-piece on this principle, and the advantage over those in ordinary use is very great. The instrument I have made and tried I have now the honour of exhibiting to the Members of the Society.

Description of a Prismatic Solar Eye-piece.

By John E. Ingpen.

(Communicated by John Browning.)

A B C is a prism, having all three surfaces polished; the angle at A is about 45° . A ray of sunlight, D E, from the object-glass of the telescope enters the prism perpendicularly to A B, and is totally reflected from E to F, where a large portion of the light and heat is refracted in the direction F I. The rest is reflected to G, and again totally reflected to H, where it emerges perpendicularly to A C, and is viewed by the eye-piece.

The larger the angle B A C (provided it is less than 60°), the greater the amount of light and heat that is got rid of.

This eye-piece is cool and pleasant in use, but the prism must be very perfect both in material and surface; and even then it is doubtful whether the finest definition can be obtained. It may, however, prove useful in certain cases.

On the Diffraction of Object-glasses. By the
Hon. J. W. Strutt, M.A., F.R.A.S.

In observing the Sun with a telescope astronomers have to adopt some device in order to obviate the injurious effects of which the intense light and heat would otherwise have on the eye. The most obvious way of doing this would be to contract the aperture of the object-glass, until the amount of light was reduced to within the necessary limit. But, as is well known, such a course cannot be followed without an enormous sacrifice of definition. The image in the focus of the object-glass of a mathematical point, is a patch of light surrounded by rings, the dimensions of the system for a given wave-length varying inversely with the diameter of the lens. If this be reduced by a diaphragm, the patches dilate, those whose centres are within a small distance overlap, and the resolving power of the telescope suffers.

It has occurred to me that the result would be quite otherwise if, instead of the marginal, the central parts of the glass were stopped off, so that the light, coming from the lens to the focus, formed a hollow cone of rays. In this case the peculiar advantage of a large aperture would not be lost, while any imperfections arising from outstanding spherical aberration would be much diminished.

The general dependence of the diffraction phenomena which occur at the focus of a telescope on the aperture and wave-length,

may be explained without mathematical analysis. Consider the centre of the image given by a well-corrected object-glass, as illuminated by secondary waves coming from every part of it. Since the phases of all these elements agree at the point in question, the illumination is a maximum, and varies as the *square* of the area of the object-glass. Now, take a neighbouring point in the focal plane. If the difference of its distances from the nearest and furthest point of the lens be but a small fraction of the wave-length of the light, the illumination must be sensibly the same as before. The distance which it is necessary to go from the centre, in order that the illumination may be diminished in a given proportion, will evidently vary directly with λ , and inversely with the diameter of the glass. If, then, we cover up the edge of the glass, the image of a point dilates, but no such effect ensues if we obstruct the central parts.

Another point seems to deserve a passing remark. Red glass is, I believe, often used to diminish the solar glare. From an optical point of view, this is the worst that could be chosen, for the ring system being proportional to λ , is the largest for red light. The wave-lengths for the fixed lines, C, D, F, are in the ratio 655:486. The substitution of a green blue light for red would, therefore, be equivalent to an enlargement of about one-third in the diameter of the object-glass.

My immediate object in the present communication is, if possible, to induce some astronomer in possession of a good telescope to make a few observations on the defining power of an object-glass provided with a central stop. The subject might be either the Sun himself, or as in Foucault's experiments (Verdet, *Leçons d'Optique Physique*, tom. I. p. 308), a scale illuminated by sunlight, and placed at a sufficient distance. Any results that might be obtained could not fail to interest physicists generally.

I append a mathematical statement of the two most contrasted cases, namely (1), when the object-glass is completely uncovered, (2) when a narrow marginal rim is alone left to act.

Let dI denote the amplitude of the vibration corresponding to a ring of the object-glass ($2\pi R dR$) at a point whose distance from the centre of the image subtends at the lens an angle θ .

Then, if x be $2\pi \frac{R \sin \theta}{\lambda}$,

$$dI = 2\pi R dR J_0(x),$$

Where

$$J_0(x) = \frac{1}{\pi} \int_0^\pi \cos(x \cos \phi) d\phi$$

is Bessel's function of order zero.

For the complete aperture from 0 to R , we have,

$$I = 2\pi \int_0^R R dR J_0\left(2\pi \frac{R \sin \theta}{\lambda}\right)$$

The intensity is represented in the two cases by the squares of dI and I . The expression for I may be written,

$$I = \frac{2\pi R^2}{x^2} \int_0^x x dx J_0(x).$$

Now, denoting differentiation by accents, $J_0(x)$ satisfies the equation

$$J_0'' + \frac{1}{x} J_0' + J_0 = 0,$$

whence,

$$\begin{aligned} \int_0^x x dx J_0(x) &= - \int_0^x dx \{x J_0'' + J_0'\} \\ &= \int_0^x dx \frac{d}{dx} \{x J_0'\} = -x J_0', \end{aligned}$$

so that,

$$I = -\pi R^2 \cdot \frac{2 J_0'(x)}{x}$$

or, since,

$$J_0'(x) = -J_1(x)$$

$$I = \pi R^2 \cdot \frac{2 J_1(x)}{x}$$

The intensities in the two cases are accordingly in the ratio

$$\left[\frac{2 J_1(x)}{x} \right]^2 : \left[J_0(x) \right]^2.$$

Let us consider, first, the positions of the dark rings. Their radii are given by the roots of the equations $J_1(x) = 0$ and $J_0(x) = 0$.

In case (2) ($J_0(x) = 0$) the values of x , corresponding to the dark rings, are

$$2.41, \quad 5.52, \quad 8.66, \quad 11.8, \quad 14.9, \quad 18.1, \text{ \&c.,}$$

being ultimately of the form $\left(m - \frac{1}{4}\right)\pi$, for the m th dark ring. These are for the pattern given by the marginal rim alone. For the whole lens we require the roots of $J_1(x) = 0$, which are

$$3.83, \quad 7.02, \quad 10.17, \quad 13.3, \quad 16.5, \quad 19.6, \text{ \&c.}$$

the m th being ultimately $\left(m + \frac{1}{4}\right)\pi$. So far the advantage lies entirely with case (2). If the size of the image be regarded as the space included within the first dark ring, the rim used alone gives a smaller image than the whole lens, in the ratio of 2.41:3.83, or about 1:1.6. From Foucault's experiments (see

Verdet), it would appear that the effective image does not extend so far as even the first dark circle. Something, however, may depend on the distribution of brightness. The functions $\frac{2J_1(x)}{x}$, $J_0(x)$ have their greatest values when $x = 0$, and are then both equal to unity. But the maxima after the first are less for $\frac{2J_1(x)}{x}$ than for $J_0(x)$. The position of the maxima of $[J_0(x)]^2$ are given by the equation $J_1(x) = 0$, and are the same as those just found for the vanishing of $\left[\frac{2J_1(x)}{x}\right]^2$.

Corresponding to the values of x ,

$$3.83, \quad 7.02, \quad 10.17, \text{ \&c.},$$

we have for $J_0(x)$ without regard to sign

$$.403, \quad .300, \quad .250, \text{ \&c.}$$

The squares of these give the maximum illuminations.

To find when $\left[\frac{2J_1(x)}{x}\right]$ is a maximum, we have by differentiation,

$$J_1'(x) - \frac{J_1(x)}{x} = 0$$

But by a property of these functions,

$$J_2 = \frac{1}{x} J_1 - J_1',$$

and, therefore, our conditions are simple,

$$J_2(x) = 0.$$

This gives for x approximately,

$$5.1, \quad 8.4, \quad 11.6, \text{ \&c.},$$

the series being ultimately the same as for $J_0(x)$, so that the $(m-1)$ th root is $\left(m - \frac{1}{4}\right)\pi$.

The corresponding values of $\frac{2J_1(x)}{x}$, without regard to sign, are

$$.13, \quad .064, \quad .040, \text{ \&c.}$$

For convenience of comparison I have drawn up a table of the maximum intensities, and the places where the maxima and minima occur for the two cases. The calculations were made by means of the tables of Bessel's functions computed by Hansen. (*Lommel, Studien ueber die Bessel'schen Functionen*).

	Whole Lens.		Marginal Rim.	
	α	Intensity.	α	Intensity.
1st max.	0°	1	0	1
1st min.	3°83	0	2°41	0
2nd max.	5°1	·017	3°83	·16
2nd min.	7°02	0	5°52	0
3rd max.	8°4	·0041	7°02	·090
3rd min.	10°17	0	8°66	0
4th max.	11°6	·0016	10°17	·057
4th min.	13°3	0	11°8	0

P.S.—Since the above paper was written, Mr. Dunkin has called my attention to a paper by the Astronomer Royal on “The Diffraction of an Annular Aperture” (*Phil. Mag.* Jan. 1841) with which I was previously unacquainted. The purport of the two papers is, however, quite different. My object was not to solve a problem in physical optics, but to direct the attention of the possessors of telescopes to the theoretical consequences of using a central stop, and to suggest that its application may possibly be found advantageous in observations where light would otherwise be in excess. There is no pretence of originality in the mathematics; indeed, the solution of the problem for an annulus is a necessary step in the treatment of the question of a lens with full aperture, the details of which are well known.

On a Volcanic Appearance in the Sun. By M. Chacornac.

(In a Letter to the President.)

“J’ai l’honneur de vous communiquer qu’hier le 29 July, vers neuf heures du matin, en faisant l’observation journalière ou habituelle de la grosse tache solaire, qui est dans sa seconde apparition, j’ai aperçu une flamme cratériforme représentant un orifice volcanique en ignition; elle était située sur l’hémisphère sud de l’astre vers le bord au trois quart du rayon, c’est-à-dire, dans une région marginale du disque solaire, occupant a-peu-près le même lieu que l’immense tache noire que nous voyons actuellement.

“Cette protuberance solaire enflammée soutendait un angle d’environ un septième du diamètre solaire et apparut colorée d’une teinte bleuâtre nettement décidée.

“Je ne puis croire à autre phénomène qu’une vision analogue à celle dite du trou D’Ulloa. Je ferai remarquer seulement ici que cette vision s’est produite alors que le Soleil élevé sur l’horizon, dépourvu de toute éclipse totale ou partielle, que le ciel était découvert de nuage, celui-ci n’était voilé ou couvert d’aucune écran; ce qui donne à cette observation ou à cette vision un caractère particulier qu’il est de mon devoir de signaler à la physiologie astronomique.

“*Ville Urbienne, le 30 Julius, 1872 (Rhône).*”

*Micrometrical Measures of ξ Ursæ Majoris, made at the Observatory of the Collegio Romano. By F. Secchi.**(Communicated by R. A. Proctor.)*

Date.	Position.	Distance.	State of the Air.	Observer.
1872.473	15°90	0°908	Bad	P. Secchi
.476	16°00	0°891	Good	"
.476	16°50		Bad	P. Ferrari
.479	13°63	1°089	Very good	"
.479	15°78	1°088	"	P. Secchi
.482	15°03	0°967	Medium	"
.482	15°08	1°051	"	P. Ferrari
.485	14°53	0°867	Bad	"

M. 1872.479	15°43	0°976	8 obs.	
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Former Observations.

Date.	Position.	Distance.	
1866.308	86°55	2°259	3 obs.
65.515	89°88	2°534	4
64.382	92°88	2°400	4
56.233	113°89	3°126	4
57.397	109°74	3°111	3

*Errata in Table of Meteor Showers, vol. xxxii.**(Communicated by Mr. Greg.)*

Page 348, No. 7, for "possibly = No. 132," read "No. 129."

Page 351, No. 59, add, "probably = No. 62."

,, No. 61, erase "ditto," referring to multiple radiant.

Page 353, No. 99, for "17° + 10°," read "17° - 10° (S.D.)."

Page 355, line 32 from top, for "is," read "or."